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PSYCHOLOGY AND PHILOSOPHY

I.—RUSSELL'S DOUBTS ABOUT INDUCTION.

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I.

A. IN the celebrated chapter on induction in his *Problems of Philosophy*, Bertrand Russell asks the question: "Have we any reason, assuming that they (laws like the law of gravitation) have always held in the past, to suppose that these laws will hold in the future?"[1] Earlier in the same chapter he raises the more specific question: "Do *any* number of cases of a law being fulfilled in the past afford evidence that it will be fulfilled in the future?"[2] We may reformulate these questions in a way which lends itself more easily to critical discussion as follows:

(1) Assuming that we possess n positive instances of a phenomenon, observed in extensively varied circumstances, and that we have not observed a single negative instance (where n is a large number), have we any reason to suppose that the $n+1$st instance will also be positive?

(2) Is there any number n of observed positive instances of a phenomenon which affords evidence that the $n+1$st instance will also be positive?

It is clear that Russell uses "reason" synonymously with "good reason" and "evidence" with "sufficient evidence". I shall follow the same procedure throughout this article.

Russell asserts that unless we appeal to a non-empirical principle which he calls the "principle of induction", both of his questions

[1] p. 100. [2] p. 96.

must be answered in the negative. "Those who emphasised the scope of induction", he writes, "wished to maintain that all logic is empirical, and therefore could not be expected to realise that induction itself, their own darling, required a logical principle which obviously could not be proved inductively, and must therefore be *a priori* if it could be known at all".[1] "We must either accept the inductive principle on the ground of its intrinsic evidence or forgo all justification of our expectations about the future".[2]

In conjunction with the inductive principle, on the other hand, question (1) at least, he contends, can be answered in the affirmative. "Whether inferences from past to future are valid depends wholly, if our discussion has been sound, upon the inductive principle: if it is true, such inferences are valid."[3] Unfortunately Russell does not make it clear whether in his opinion the same is true about question (2).

As against Russell, I shall try to show in this article that question (1) can be answered in the affirmative without in any way appealing to a non-empirical principle. I shall also attempt to show that, without in any way invoking a non-empirical principle, numbers of observed positive instances do frequently afford us evidence that unobserved instances of the same phenomenon are also positive. At the outset, I shall concentrate on question (1) since this is the more general question. Once we have answered question (1) it will require little further effort to answer question (2).

I want to emphasise here that, to keep this paper within manageable bounds, I shall refrain from discussing, at any rate explicitly, the questions "Are any inductive conclusions probable?" and "Are any inductive conclusions certain?" I hope to fill in this gap on another occasion.

It will be well to conduct our discussion in terms of a concrete example. Supposing a man jumps from a window on the fiftieth floor of the Empire State Building. Is there any reason to suppose that his body will move in the direction of the street rather than say in the direction of the sky or in a flat plane? There can be no doubt that any ordinary person and any philosophically unsophisticated scientist, would answer this question in the affirmative without in any way appealing to a non-empirical principle. He would say that there is an excellent reason to suppose that the man's body will move towards the street. This

[1] *Our Knowledge of the External World* (2nd edition), p. 226.
[2] *Problems of Philosophy*, p. 106; also *Outline of Philosophy*, p. 286.
[3] *External World*, p. 226.

excellent reason, he would say, consists in the fact that whenever in the past a human being jumped out of a window of the Empire State Building his body moved in a downward direction; that whenever any human being anywhere jumped out of a house he moved in the direction of the ground; that, more generally, whenever a human body jumped or was thrown off an elevated locality in the neighbourhood of the earth, it moved downwards and not either upwards or at an angle of 180°; that the only objects which have been observed to be capable of moving upwards by themselves possess certain special characteristics which human beings lack; and finally in all the other observed confirmations of the theory of gravitation.

B. The philosophers who reject commonsense answers like the one just described, have relied mainly on three arguments. Russell himself explicitly employs two of them and some of his remarks make it clear that he also approves of the third. These three arguments are as follows: (*a*) Defenders of commonsense point to the fact that many inferences to unobserved events were subsequently, by means of direct observation, found to have resulted in true conclusions. However, any such appeal to observed results of inductive inferences is irrelevant. For the question at stake is: Have we ever a reason, assuming that all the large number of observed instances of a phenomenon are positive, to suppose that an instance which is still unobserved is also positive? The question is not: Have we ever a reason for supposing that instances which have by now been observed but were at one time unobserved are positive? In Russell's own words: "We have experience of past futures, but not of future futures, and the question is: Will future futures resemble past futures? This question is not to be answered by an argument which starts from past futures alone." [1]

(*b*) Cases are known where at a certain time a large number of positive instances and not a single negative instance had been observed and where the next instance nevertheless turned out to be negative. "We know that in spite of frequent repetitions there sometimes is a failure at the last." [2] The man, for instance, "who has fed the chicken every day throughout its life at last wrings its neck instead." [3] Even in the case of the human being who is jumping out of the Empire State Building, "we may be in no better position than the chicken which unexpectedly has its neck wrung." [4]

(*c*) The number of positive and negative necessary conditions

[1] *Problems of Philosophy*, p. 100.
[2] *loc. cit.*, p. 102.
[3] *loc. cit.*, p. 98.
[4] *Ibid.*

for the occurrence of any event is infinite or at any rate too large to be directly observed by a human being or indeed by all human beings put together. None of us, for example, has explored every corner of the universe to make sure that there nowhere exists a malicious but powerful individual who controls the movements of the sun by means of wires which are too fine to be detected by any of our microscopes. None of us can be sure that there is no such Controller who, in order to play a joke with the human race, will prevent the sun from rising to-morrow. Equally, none of us can be sure that there is nowhere a powerful individual who can, if he wishes, regulate the movement of human bodies by means of ropes which are too thin to be detected by any of our present instruments. None of us therefore can be sure than when a man jumps out of the Empire State Building he will not be drawn skyward by the Controller of Motion. Hence we have no reason to suppose that the man's body will move in the direction of the street and not in the direction of the sky.

In connexion with the last of these three arguments attention ought to be drawn to a distinction which Russell makes between what he calls the "interesting" and the "uninteresting" doubt about induction.[1] The uninteresting doubt is doubt about the occurrence of a given event on the ground that not all the conditions which are known to be necessary are in fact known to be present. What Russell calls the interesting doubt is the doubt whether an event will take place although all the conditions known to be necessary are known to obtain. Russell's "interesting doubt", if I am not mistaken, is identical with Donald Williams's "tragic problem of induction".[2]

II.

As I indicated above, it is my object in this article to defend the commonsense answers to both of Russell's questions. I propose to show, in other words, that, without in any way calling upon a non-empirical principle for assistance, we often have a reason for supposing that a generalisation will be confirmed in the future as it has been confirmed in the past. I also propose to show that numbers "of cases of a law being fulfilled in the past" do often afford evidence that it will be fulfilled in the future.

However, what I have to say in support of these answers is so exceedingly simple that I am afraid it will not impress the

[1] *loc. cit.*, p. 95. [2] *Induction and the Future*, MIND, 1948, p. 227.

philosophers who are looking for elaborate and complicated theories to answer these questions. But I think I can make my case appear plausible even in the eyes of some of these philosophers if I describe at some length the general method of resolving philosophical puzzles which I shall apply to the problem of induction.

Let us consider a simple statement like "there are several thousand physicians in New York". We may call this a statement of commonsense, meaning thereby no more than that anybody above a certain very moderate level of instruction and intelligence would confidently give his assent to it.

The word "physician", as ordinarily used, is not entirely free from ambiguity. At times it simply means "person who possesses a medical degree from a recognised academic institution". At other times, though less often, it means the same as "person who possesses what is by ordinary standards a considerable skill in curing diseases". On yet other occasions when people say about somebody that he is a physician they mean both that he has a medical degree and that he possesses a skill in curing diseases which considerably exceeds that of the average layman.

Let us suppose that in the commonsense statement "there are several thousand physicians in New York" the word "physician" is used exclusively in the last-mentioned sense. This assumption will simplify our discussion, but it is not at all essential to any of the points I am about to make. It is essential, however, to realise that when somebody asserts in ordinary life that there are several thousand physicians in New York, he is using the word "physician" in one or other of the ordinary senses just listed. By "physician" he does not mean for example "person who can speedily repair bicycles" or "person who can cure any conceivable illness in less than two minutes".

Now, supposing somebody were to say "Really, there are no physicians at all in New York", in the belief that he was contradicting and refuting commonsense. Supposing that on investigation it turns out that by "physician" he does not mean "person who has a medical degree and who has considerably more skill in curing disease than the average layman". It turns out that by "physician" he means "person who has a medical degree and who can cure any conceivable illness in less than two minutes".

What would be an adequate reply to such an "enemy of commonsense"? Clearly it would be along the following lines: "What you say is true. There are no physicians in New York—in *your* sense of the word. There are no persons in New York

who can cure any conceivable disease in less than two minutes. But this in no way contradicts the commonsense view expressed by "there are several thousand physicians in New York". For the latter asserts no more than that there are several thousand people in New York who have a medical degree and who possess a skill in curing disease which considerably exceeds that of the average layman. You are guilty of *ignoratio elenchi* since the proposition you refute is different from the proposition you set out to refute."

Our discussion from here on will be greatly simplified by introducing a few technical terms. Let us, firstly, call "*ignoratio elenchi* by *redefinition*" any instance of *ignoratio elenchi* in which (i) the same sentence expresses both the proposition which ought to be proved and the proposition which is confused with it and where (ii) in the latter employment of the sentence one or more of its parts are used in a sense which is different from their ordinary sense or senses. Secondly, let us refer to any redefinition of a word which includes all that the ordinary definition of the word includes but which includes something else as well as a "*high* redefinition"; and to the sense which is defined by a high redefinition we shall refer as a high sense of the word. Thus "person who has a medical degree and who is capable of curing any conceivable disease in less than two minutes" is a high redefinition of "physician" and anybody using the word in that fashion is using it in a high sense. Thirdly, we shall refer to a redefinition of a word which includes something but not all of what the ordinary definition includes and which includes nothing else as a "*low* redefinition"; and the sense which is defined by a low redefinition we shall call a low sense of the word. "Person capable of giving first aid" or "person who knows means of alleviating pain" would be low redefinitions of "physician". Finally, it will be convenient to call a statement in which a word is used in a high or in a low sense a *redefinitional statement*. If the word is used in a high sense we shall speak of a highdefinitional statement; if it is used in a low sense we shall speak of a lowdefinitional statement.

A short while ago, I pointed out that the man who says "there are no physicians in New York", meaning that there are no people in New York who have a medical degree and who can cure any conceivable illness in less than two minutes, is not really contradicting the commonsense view that there are physicians in New York. I pointed out that he would be guilty of what in our technical language is called an *ignoratio elenchi* by redefinition. Now, it seems to me that the relation between the assertion of

various philosphers that past experience never constitutes a reason for prediction or generalisation except perhaps in conjunction with a non-empirical principle and the commonsense view that past experience does often by itself constitute a reason for inferences to unobserved events has some striking resemblances to the relation between the redefinitional statement about physicians in New York and the commonsense view which this redefinitional statement fails to refute. And more generally, it strongly seems to me that almost all the bizarre pronouncements of philosophers—their "paradoxes", their "silly" theories—are in certain respects strikingly like the statement that there are no physicians in New York, made by one who means to assert that there are no people in New York who have medical degrees and who are capable of curing any conceivable disease in less than two minutes.

In making the last statement I do not mean to deny that there are also important differences between philosophical paradoxes and the highdefinitional statement about physicians. There are three differences in particular which have to be mentioned if my subsequent remarks are not to be seriously misleading. Firstly, many of the philosophical paradoxes are not without some point; they do often draw attention to likenesses and differences which ordinary usage obscures. Secondly, the redefinitions which are implicit in philosophical paradoxes do quite often, though by no means always, receive a certain backing from ordinary usage. Frequently, that is to say, there is a secondary sense or trend in ordinary usage which corresponds to the philosophical redefinition, the "real" sense of the word.[1] Thirdly, philosophical paradoxes are invariably ambiguous in a sense in which the highdefinitional statement about the physicians is not ambiguous.[2]

Now, while fully admitting all these (and other) differences, I wish to insist on the great likenesses between philosophical paradoxes and the redefinitional statement about the physicians. And in this article I am mainly concerned with the likenesses, not with the differences. My main object of course is to point out the likenesses between the highdefinitional statement "there are no physicians in New York" and the statement that past experience never by itself affords a reason for making inferences to

[1] Prominent instances of this phenomenon are "real certainty", "real knowledge", "real sameness", "real freedom", and "really contemporaneous events".

[2] The last of these points seems to me to be of enormous importance for understanding the phenomenon of philosophical paradoxes.

unobserved events. However, my points there will be clearer if I first make them in connexion with another celebrated paradox.

Following Plato, Berkeley [1] argued in favour of the view that heat and cold are not really " in the object ". Ordinary people would unhesitatingly say that water of *e.g.*, 50° Centigrade is hot. Against this, Plato and Berkeley would point out that to a man who a moment before had held his hands in a jug of water with a temperature of 80° C., the water of 50° C. would appear cold. Similarly, to a race of individuals whose body-temperature was say 75° C., water of 50° would regularly appear cold. But the percepts of those to whom the water of 50° appears cold are just as genuine as the percepts of people to whom the water appears hot. Now, since it would be wrong to say that the water of 50° is really cold simply because of these genuine percepts of cold, it cannot any more rationally be said to be hot. The cold has " just as good a right to be considered real " as the hot ; and therefore, " to avoid favouritism, we are compelled to deny that in itself " [2] the water is either hot or cold.

It is not difficult to show that this argument is a case of *ignoratio elenchi* by redefinition. When an ordinary person says that water of 50° C. is hot all he means is that human beings, with their body-temperature being what it is, would in *all ordinary circumstances* have sense-impressions of heat on coming into contact with such water. In saying that water of 50° is hot, is *really* hot, an ordinary person in no way denies that under certain *special* conditions a human being would have genuine sense-impressions of cold. He also in no way denies that to a race of individuals whose body-temperature is 75° the water would genuinely appear cold. Pointing to these facts does therefore not refute the ordinary man. Berkeley is clearly guilty of a high redefinition of " hot " or " really hot ". To him something is hot only if, in addition to appearing hot to human beings in ordinary circumstances, it also appears hot to them under special circumstances and if it appears hot to beings with a body-temperature which is much greater than the actual body-temperature of human beings.

However, this is not quite accurate since, like most other philosophical paradoxes, the paradox about heat and cold has a double meaning. It would be inaccurate simply to say that Berkeley is guilty of *ignoratio elenchi* by redefinition. On the

[1] *Three Dialogues between Hylas and Philonous*, p. 208 (Everyman edit.).

[2] The phrases are Russell's, used in a very similar context (*Problems*, p. 14).

other hand, without in any way being inaccurate, it can be said that Berkeley and Plato have laid themselves open to the following dilemma : " Either you mean by ' hot ' what is ordinarily meant by it—if you do, then what you say is plainly false ; or else you are using ' hot ' in a high sense—if so what you say is true, but in that case you are guilty of *ignoratio elenchi* by redefinition. In either event you have failed to refute commonsense." Very similar answers can also be made to Berkeley's and Russell's arguments concerning colours, shapes, and the other qualities which commonsense believes to exist independently of being perceived.

At the same time it must be admitted that Berkeley's arguments have a certain value. In ordinary speech we make a fairly rigid distinction between " real " and " unreal " data. Among the unreal data we lump together both the percepts which we have under special conditions (and percepts which do and would appear to beings differently constituted from ourselves) and what we experience *e.g.*, in dreams and hallucinations. " Real " we call only those percepts which a normal observer has under certain standard conditions.

A classification of this sort obscures the many likenesses between the " real " percepts and percepts appearing under special conditions, while also hiding the many differences between the latter and data which are experienced in dreams and hallucinations.

The situation becomes quite clear if we divide data into three and not merely into two groups, as follows :

the R-data : percepts appearing to a normal observer under standard conditions,

the A-data : percepts appearing to a normal observer under special conditions or to an abnormal observer in certain normal or special circumstances, and

the D-data : data appearing in dreams, hallucinations, etc.

It is unnecessary for our purposes to discuss exactly what are the likenesses between the R-data and the A-data. It is unnecessary, too, to discuss what exactly are the differences between the A-data and the D-data. It is sufficient to point out that while Berkeley is wrong in believing or suggesting that there are no differences between the R-data and the A-data, he is right in insisting that the differences between the R-data and the A-data are not nearly as great as ordinary speech suggests. In the case of colours, Berkeley's argument has the further merit of bringing out the fact that the expression " X's real colour " has *two*

perfectly proper senses. His argument helps one to realise that " X's real colours " may mean " the colour which X exhibits to a normal observer under certain standard conditions " *as well as* " the colour which X exhibits to a normal observer under a finer instrument than the human eye, *e.g.*, a microscope".

III.

A. Supposing a man, let us call him M, said to us " I have not yet found any physicians in New York ". Suppose we take him to Park Avenue and introduce him to Brown, a man who has a medical degree and who has cured many people suffering from diseases of the ear. Brown admits, however, that he has not been able to cure *all* the patients who ever consulted him. He also admits that many of his cures took a long time, some as long as eight years. On hearing this, M says " Brown certainly isn't a physician ".

Supposing we next take M to meet Black who has a medical degree and who can prove to M's and to our satisfaction that he has cured every patient who ever consulted him. Moreover, none of Black's cures took more than three years. However, on hearing that some of Black's cures took as long as two years and ten months, M says " Black certainly isn't a physician either ".

Finally we introduce M to White who has a medical degree and who has cured every one of his patients in less than six months. When M hears that some of White's cures took as long as five and a half months, he is adamant and exclaims " White—what a ridiculous error to call him a physician ! "

At this stage, if not much sooner, all of us would impatiently ask M : What on earth do you mean by " physician " ? And we would plainly be justified in adding : Whatever you may mean by " physician ", in any sense in which we ever use the word, Black and Brown and White are physicians and very excellent ones at that.

Let us return now to Russell's doubt about the sun's rising to-morrow or about what would happen to a man who jumps out of the Empire State Building. Let us consider what Russell would say in reply to the following question : Supposing that the observed confirmatory instances for the theory of gravitation were a million or ten million times as extensive as they now are and that they were drawn from a very much wider field ; would we then have a reason to suppose that the man will fall into the street and not move up into the sky? It is obvious that Russell

and anybody taking his view would say " No ". He would reply that though our *expectation* that the man's body will move in the direction of the street would be even stronger then than it is at present, we would still be without a *reason*.

Next, let us imagine ourselves to be putting the following question to Russell: Supposing the world were such that no accumulation of more than five hundred observed positive instances of a phenomenon has ever been found to be followed by a negative instance; supposing, for instance, that all the chickens who have ever been fed by the same man for 501 days in succession or more are still alive and that all the men too are still alive feeding the chickens every day—would the observed confirmations of the law of gravity in that case be a reason to suppose that the man jumping out of the Empire State Building will move in the direction of the street and not in the direction of the sky? I am not quite sure what Russell would say in reply to this question. Let us assume he would once again answer " No—past experience would not even then ever be a *reason* ".

Thirdly and finally, we have to consider what Russell would say to the following question: Supposing we had explored every corner of the universe with instruments millions of times as fine and accurate as any we now possess and that we had yet failed to discover any Controller of the movements of human bodies—would we then in our predictions about the man jumping out of the Empire State Building be in a better position than the chicken is in predicting its meals? Would our past observations then be a reason for our prediction? Whatever Russell would in fact say to this, it is clear that his remarks concerning the " interesting " doubt about induction require him to answer our question in the negative. He would have to say something like this: " Our *expectation* that the man's body will move in a downward direction will be even stronger than it is now. However, without invoking a non-empirical principle, we shall not *really* be in a better position than the chicken. We should still fail to possess a *reason*."

As in the case of the man who refused to say that Brown, Black, and White were doctors, our natural response to all this will be to turn to Russell and say: What do you mean by " being in a better position "? What on earth do you mean by " a reason "? And, furthermore, why should anybody be interested in a reason in your sense of the word?

Russell's remarks about the need for a general principle like his principle of induction to serve as major premiss in every inductive argument make it clear what he means by a reason: like the Rationalists and Hume (in most places), he means by " reason "

a *logically conclusive* reason and by " evidence " *deductively conclusive* evidence. When " reason " is used in this sense, it must be admitted that past observations can never by themselves be a reason for any prediction whatsoever. But " reason " is not used in this sense when, in science or in ordinary life, people claim to have a reason for a prediction.

So far as I can see, there are three different trends in the ordinary usage of " reason for an inductive conclusion " and according to none of them does the word mean " logically conclusive reason". Among the three trends one is much more prominent than the others. It may fitly be called the main sense of the word. According to this main sense, what we mean when we claim that we have a reason for a prediction is that the past observations of this phenomenon or of analogical phenomena are of a certain kind : they are exclusively or predominantly positive, the number of the positive observations is at least fairly large, and they come from extensively varied sets of circumstances. This is of course a very crude formulation. But for the purposes of this article it is, I think, sufficient.[1]

Next, there is a number of trends according to which we mean very much less than this. Occasionally, for instance, we simply mean that it is *reasonable* to infer the inductive conclusion. And clearly it may be reasonable to infer an inductive conclusion for which we have no reason in the main sense. Thus let us suppose I know that Parker will meet Schroeder in a game in the near future and that it is imperative for me not to suspend my judgment but to come to a conclusion as to who will win. Supposing I know nothing about their present form and nothing also about the type of court on which the match is to be played. All I know is that Parker and Schroeder have in the previous two seasons met six times, Parker scoring four victories to Schroeder's two. In these circumstances it would be reasonable for me to predict that Parker will win and unreasonable to predict that Schroeder will win. Clearly however, in the main sense of the word I have no reason for either prediction.

Again there is a trend according to which any positive instance of a phenomenon is *a* reason for concluding that the next instance of the phenomenon will be positive. Thus in the circumstances described in the preceding paragraph, it would be quite proper to say we have *more reason* for supposing that Parker will win than for predicting Schroeder's victory. It would be quite proper also

[1] I have so far left out one important element in the main sense of " reason for an inductive conclusion ". I shall come to that in Section IV. In the meantime this omission will not affect any of my points.

to say that we have *some reason* for supposing that Schroeder will win. It would be proper to say this even if Schroeder had won only one of the six matches. To all these and similar trends in the ordinary usage of " reason for an inductive conclusion " I shall from now on refer as the second ordinary sense of the word.

There can be no doubt that in both these ordinary senses of the word, we frequently have a reason for an inductive conclusion. In these senses we have an excellent reason for supposing that the man jumping out of the Empire State Building will move in the direction of the street, that the sun will rise to-morrow and that Stalin will die before the year 2000. The answer to question (1) is therefore a firm and clear " Yes " : in many domains we have a multitude of exclusively positive instances coming from extensively different circumstances.

The same is true if " reason " is used in the third ordinary sense. However, I propose to reserve our discussion of that sense for Section V below. For the time being it will be convenient and, I think, not at all misleading to speak as if what I have called the main sense is the *only* ordinary sense of " reason for an inductive conclusion ".

It should now be clear that, when Russell says that observed instances are never by themselves a reason for an inductive conclusion, he is guilty of an *ignoratio elenchi* by redefinition. His assertion that the premisses of an inductive argument never by themselves constitute a *logically conclusive* reason for an inductive conclusion in no way contradicts the commonsense assertion that they frequently constitute a reason *in the ordinary sense of the word*. Russell's definition of " reason " is indeed in one respect not a redefinition since in certain contexts we do use " reason " to mean " deductively conclusive reason ". However, it is a redefinition in that we never in ordinary life use " reason " in Russell's sense when we are talking about inductive arguments.

Moreover, if " reason " means " deductively conclusive reason ", Russell's questions are no more genuinely questions than *e.g.*, the sentence " Is a father a female parent ? " For, since part of the definition of " inductive inference " is inference from something observed to something unobserved, it is a *contradiction* to say that an inference is both inductive and at the same time in the same respect deductively conclusive. Russell's " interesting " doubt, then, is no more sensible or interesting than the " doubt " whether we shall ever see something invisible or find an object which is a father and also female or an object which is a man but not a human being.

In a similar fashion, Russell's remarks about the future future

which we quoted in Section 1B constitute an *ignoratio elenchi* by redefinition.[1] If the word "future" is used in its ordinary sense in the statement "the future will resemble the past and the present in certain respects" then we have plenty of evidence to support it. For in the ordinary sense of the word, "future" simply means "period which has to the past and the present the relation of happening after it". In its ordinary sense, "future" does *not* mean "period which has to the past and the present the relation of happening after it *and* which can never itself be experienced *as a present*". The period which is referred to by "future" in its ordinary sense may very well one day be experienced as a present.

In the ordinary sense of the word "future" therefore, what Russell calls past futures *are* futures. They are futures in relation to certain other periods which preceded them. Now, the appeal to the fact that past futures resembled past pasts and past presents constitutes excellent inductive evidence for the conclusion that the future will resemble the past and the present. Stated fully, the argument is as follows: a period which has to the past and present the relation of happening after it will resemble the past and the present in certain respects because in the past periods which stood in the same temporal relation to other periods were found to resemble those periods in these respects.

It should be emphasised that in the conclusion of this argument "future" means "future future", as that phrase would normally be understood. It refers to a period which by the time at which the statement is made has not yet been experienced, *i.e.*, has not yet become a present or a past.

The appeal to the resemblance between past futures and past pasts and presents is not to the point only if in the sentence "the future will resemble the past and the present" the word "future" means "period which has to the present the relation of occurring after it *and* which can never be experienced as a present". In that case, of course past futures are not really futures. For, when they were experienced they were experienced as presents. However, anybody who in ordinary life or in science says or implies that the future will resemble the past and the present does not use "future" in this sense. He means to assert something about a future which may one day be experienced as a present.

B. If Russell had answered in the affirmative any of the three questions which we imagined ourselves to be addressing to him,

[1] The paragraphs which follow are a summary in my own words of the main point of F. L. Will's delightful article "Will the Future be like the Past?" (MIND, 1947).

his question (1) would be a genuine question in the sense that it could then not be disposed of by an examination of definitions alone. But even then Russell would have been guilty of *ignoratio elenchi* by high redefinition. For in order to have a reason, in the ordinary sense of the word, for inferring that the next instance of a certain phenomenon is positive it is not necessary to observe all the positive and negative necessary conditions for the occurrence of this instance. Nor is it necessary that the collection of positive observed instances should be larger or taken from more extensively different circumstances than many we actually have. Nor, finally, is it necessary that breakdowns should never have occurred in *any* domain. All that is necessary in this connexion is that there should have been no breakdowns in the same domain. Or if any did occur in the same domain they must have proved capable of correlation with certain special features which are known not to be present in the subject of the prediction.

Anybody who takes the trouble to observe the ordinary usage of the word " reason " in connexion with inductive arguments can easily check up on these claims.

It may be interesting to return for a moment to the case of the chicken which finally had its neck wrung. If we had explored every corner of the universe with wonderfully fine instruments and failed to discover a Controller of human movements, then in any ordinary sense of " being in a better position " we should undoubtedly, be in a better position in the case of the man jumping out of the Empire State Building than the chicken in regard to its meals. If Russell even then denied that we are in a better position he is surely using the phrase " being in a better position " in a strange sense. Or else he is asserting a very plain falsehood. For to say that possession of one set of observed facts, say P, puts one in a better position with regard to a certain inductive conclusion, say *c*, than possession of another set of observed facts, say Q, simply means that P is a reason for *c* while Q is not, or that P is a better reason than Q.

Moreover, even without having explored every corner of the universe, we *are* in a very much better position in the case of predicting the sun's rising or the movement of a man jumping from the Empire State Building than the chicken is regarding its meals. The truth is that Russell's analogy, although it is not wholly pointless, is very weak indeed. Its only merit consists in bringing out the fact that neither we nor the chicken have explored every corner of the universe. On the other hand, there are two important differences which Russell obscures when he

says that even in the case of our most trusted scientific theories we may be in no better a position than the chicken. Firstly, the number of observed instances supporting our prediction in a case like the man's jumping from the Empire State Building is obviously much greater than the number of positive instances observed by the chicken. And secondly, although we cannot definitely say that there is nowhere a Controller of human motions, we certainly have no reason whatsoever to suppose that one exists. We have no reason whatsoever to suppose that a living individual, in any ordinary sense of " control ", controls the movements of human beings who jump out of a house. The chicken, on the other hand, if it knows anything, knows that it depends for its meals on another living object.

C. Let us now turn to question (2) : Is there any number, n, of observed positive instances of a phenomenon which affords evidence that the n + 1st instance will also be positive ? I have already mentioned the familiar fact that scientists as well as ordinary people of a certain level of intelligence do not rely for their inductive conclusions on the number of observed positive instances exclusively. However, it will be easier to discuss the question before us if we proceed on the assumption that according to commonsense the strength of past experience as evidence depends on the number of observed positive instances and on nothing else. All important points can be made more easily if we proceed on this assumption.

Now, in two senses the answer to question (2) must be admitted to be a clear " No ". Firstly, even if there were in every domain or in some domains a number of observed positive instances which constitutes the dividing line between evidence and non-evidence or, as it is more commonly expressed, between sufficient and insufficient evidence, there is no reason whatsoever to suppose that the number would be the same for different domains. There is no reason to suppose that in the domain of animal learning, for example, the number is the same as in the domain of the movements of the heavenly bodies. But, secondly, there is no such number in *any* domain. For we are here clearly faced with a case of what is sometimes called " continuous variation ". There is no more *a* number dividing sufficient from insufficient evidence than there is a number dividing bald people from those who are not bald or poor people from people who are not poor.

These facts, however, imply nothing against commonsense. For, from the fact that there is no rigid division between sufficient and insufficient evidence it does not follow that there are no cases of sufficient evidence. From the fact that there is no number

which constitutes the borderline between adequate collections of positive instances and those which are not adequate it does not follow that no number of positive instances is adequate. Although we cannot point to a number which divides bald people from people who are not bald, we can without any hesitation say that a man without a single hair on his head is bald while one with a million hairs on his head is not bald.

Furthermore, just as we can say about many people that they are bald and about many others that they are not bald although we have not counted the number of hairs on their heads and just as we can say that Rockefeller is rich although we cannot even approximately say what is the dollar-equivalent of his total possessions, so we can very often say *that* a number of observed instances constitutes sufficient evidence although we cannot say *what* this number is. The number of instances supporting the theory of gravitation which human beings have observed is for example more than sufficient evidence—in any ordinary sense of the word—for supposing that the man jumping out of the Empire State Building will move in a downward direction. But nobody knows what this number is. Human beings simply do not bother to keep records of all instances which confirm the law of gravity.

IV.

A few words must now be said about the claim, made by Russell, Ewing and others, that empiricism cannot provide a justification of induction since any inductive or empirical justification of induction would necessarily beg the question. If the principle of induction "is not true", to use Russell's words, "every attempt to arrive at general scientific laws from particular observations is fallacious, and Hume's scepticism is inescapable for an empiricist". But "the principle itself cannot, without circularity, be inferred from observed uniformities, since it is required to justify any such inference".[1]

In the light of our remarks about redefinitions it is easy to see that all claims of this nature are either mistaken or else cases of *ignoratio elenchi* by redefinition. Before showing this, it will be well to restate the principle of induction in a form which is less confusing than that which Russell uses. Let us try the following formulation:

[1] *History of Western Philosophy*, p. 699.

> The greater the number of positive instances of a phenomenon which have been observed, assuming that no or none except easily explicable negative instances have been found, *and* the greater the number of kinds from which the positive instances are drawn, the less often does it happen that a new instance of the phenomenon turns out to be negative.[1]

I admit that this statement is rather vague and I also admit that, unless one qualifies it so as to deprive it of all factual significance, one can find exceptions to it.

At the same time, it seems plain that the principle as here stated is very much closer to the truth than its contrary. Furthermore, whether or not it would be correct to regard the inductive principle as a *premiss* of all inductive arguments, it does seem to me part of the *reason* for every inductive conclusion. I mean by this that we would not apply " reason " to a large number of positive and widely varied instances if the contrary of the inductive principle were true or nearer the truth than the inductive principle. Supposing, for example, it had been found in all domains that after 10,000 instances had been observed, all of them positive and gathered from very varied circumstances, chaos was found among the rest. After the 10,000th instance, in other words, predictions always became thoroughly unreliable. Supposing that in these circumstances we discover a new species of animal—let us call them grats. We want to find how long it takes the grats to solve a certain puzzle and find that all our first 10,000 subjects can solve it in less than an hour. Would we say, knowing what happened in all the many observed domains after the 10,000th instance, we had a reason for supposing that the 10,001st grat would also solve the puzzle in less than an hour ? It seems clear that most of us would refuse to say this.

It is now apparent that my analysis in Section III of the main sense and also of the second ordinary sense of " reason for an inductive conclusion " was incomplete. It will be sufficient here to indicate how my analysis requires to be supplemented in the case of the main sense. To say that p is a reason for an inductive conclusion, in the main sense of " reason ", is to say firstly that part of p asserts what I earlier claimed the whole of p to assert *and* secondly that the rest of p asserts the inductive principle. Part of p asserts the inductive principle at least in the sense of asserting that it is much closer the truth than its contrary.

Miss Ambrose, in her splendid article on induction, has tried to meet the charge of *petitio principii* by contending that the

[1] *cf.* Ernest Nagel, *Principles of the Theory of Probability*, p. 72.

principle of induction is not a premiss of inductive arguments, but a principle of inference or substitution *according* to which "inductive inferences are made".[1] But this seems to me an inadequate reply to the charge. For the enemies of commonsense might admit that what Miss Ambrose says is true of the principle as Russell is in the habit of formulating it. But they might then proceed to restate it in some such way as I have done, maintaining that in this sense it does form part of the reason for every inductive conclusion. At this stage they would undoubtedly renew their charge that the inductive argument cannot be supported by an inductive argument without begging the question.

And I want to show now that my admission that the inductive principle is part of the reason for every inductive conclusion implies nothing against commonsense or against empiricism. For this purpose it is necessary to distinguish two possible senses of any statement of the form "All S are P". Such a statement may either mean "All *observed* S are P"; or it may mean "All S *whatsoever* are P". I propose to refer to statements of the first class as "universal premisses" and to statements of the second class as "universal conclusions". Now, the charge of *petitio principii* could be sustained only if the inductive principle were meant as a universal *conclusion* when forming part of the evidence of inductive conclusions. But it is clear that when it forms part of the evidence of inductive conclusions, the inductive principle is or requires to be meant only as a universal *premiss*. We would refuse to regard a large collection of exclusively positive and widely varied instances of a phenomenon as a good reason for predicting that the next instance will also be positive if in all or most previous cases large collections of exclusively positive and widely varied instances turned out to be a thoroughly unreliable basis for prediction. However, given a large collection of exclusively positive and widely varied instances of a phenomenon, it would be sufficient for a correct application of "reason" that in all or most *observed* cases large collections of exclusively positive and widely varied instances turned out to be a reliable basis for prediction. Any opinion to the contrary rests on the belief, exploded in the previous section, that according to ordinary usage "reason for an inductive conclusion" means "deductively conclusive reason for the inductive conclusion".

[1] "The Problem of Justifying Inductive Inference", *Journal of Philosophy*, 1947, pp. 260 ff. Miss Ambrose's point is not actually made in order to answer the charge of *petitio principii*. However, if what she says were true of all possible forms of the inductive principle, the charge would have been implicitly disposed of.

V.

I can well imagine that some people will not be moved by what I have said. Even if Russell himself were convinced, there are undoubtedly other philosophers who would take me to task for evading what they would declare to be the real issue. " You may have shown," it would be said, " that in the ordinary sense of ' reason ' and ' evidence ' past observations do often constitute a good reason and sufficient evidence. But how do you know that what is a reason in the ordinary sense is *really* a *reason?* The fact that the sun has risen every day so far is admittedly a reason, in the ordinary sense, for supposing that it will again rise to-morrow. For to say this is simply to say that it has always risen in the past. But *can you predict* that the sun will again rise to-morrow simply because it has always risen in the past ? The question, the interesting doubt about induction in this instance is not : Have we any reason in the ordinary sense for supposing that the sun will rise to-morrow ? To this, we agree, the answer is " Yes ". The real question is : Having a reason, in the ordinary sense, for believing that the sun will rise to-morrow, can we infer from this with any reliability that the sun will again rise to-morrow ?

Before I take up this objection I should like to fill in a gap in my analysis of the ordinary usage of the phrase " reason for an inductive conclusion ". It will be remembered that in Section III I distinguished between three trends in the ordinary usage of this phrase. Firstly there is what I called the main sense of the word ; secondly there is a set of trends which I grouped together as the second sense of the word ; and finally there is a trend or sense to which I alluded but which I have so far not attempted to analyse. According to both senses I analysed, " p constitutes a reason for c " (where c stands for some inductive conclusion) asserts the existence of *observed* events exclusively. Its truths need not at all be affected by the discovery that c is false.

Now, the third sense which I have not yet analysed is much less prominent than the main sense but, so far as I can see, much more prominent than the trends which I have grouped together as the second sense. When " reason " is used in this third sense the observed facts referred to by " reason " in the main sense are part of its referent, but they are not the whole of it. It is not indeed a necessary condition for the application of " reason " (in this sense) to a set of propositions, say p, that the prediction based on p be *true*. But, where the prediction refers to a multitude of events, it is a necessary condition that it be considerably

nearer the truth than its contrary. Where the prediction explicitly refers to a single event only, it is a necessary condition that a considerable majority of instantial predictions having the same relation to p be true. Thus, according to the third sense, we would have had a reason for believing that the man jumping out of the Empire State Building will move in a downward direction although subsequent observation shows him to move into the sky—provided that in most other cases, as yet unobserved at the time of making our prediction, human bodies did in similar circumstances move downwards. With our large collection of exclusively positive and widely varied past instances, we would have had a reason for believing that all men who will jump out of houses are going to move in a downward direction even if a few of them disappeared in the sky so long as *most* of them moved as we predicted. We would have had no reason in this third sense if in the case of a large proportion of subsequent jumps—approaching half the total number of new jumps—bodies failed to move in a downward direction.

It will be helpful to use different signs to distinguish between a reason in the main and a reason in the third sense. Let us use the sign "reason m" to stand for reasons in the main sense and the sign "reason f" to signify reasons in the third sense. Using this terminology, we may restate the objection outlined at the beginning of the present section as follows: "You have shown that frequently people have reasons m for believing in inductive conclusions. However, the real question is whether, without appealing to a non-empirical principle, they ever have reasons f; and this you have not shown". I could have stated this charge more easily by using the words "probable" and "certain". But, as I explained earlier, an explicit discussion of the questions "Are any inductive conclusions probable"? and "Are any inductive conclusions certain"? is beyond my scope.

In reply to this charge, I wish to make two comments. The first of these is as follows: it simply is a fact that, given certain sets of observations, human beings can make true predictions. It simply is a fact that given reasons in the sense of reason m we very often also have reasons in the sense of reason f. This is a fact just as it is a fact that human beings can make genuine observations and just as it is a fact that certain objects have certain spatial relations to one another and that some events happen after other events. It is logically and also I think factually possible to have feelings of doubt and anxiety concerning the outcome of any prediction whatsoever. But it is also possible to have such doubts about the genuineness of observations at

the present moment and about the reality of spatial and temporal relations. The possibility or the actual existence of such feelings no more implies that human beings cannot in certain circumstances make true predictions than it implies that they never make genuine observations or that there are no real relations in space and time.

Secondly, it seems to me that a person who has all the information which ordinary mortals have but who nevertheless asks, with an air of infinite puzzlement " How can we now predict something which is not yet ? " is tacitly confusing the statement " *c* is true " with the statement "*c* has been or is being directly verified ".[1] " *c* can now be correctly predicted " does indeed imply " *c* is true ", but it does not imply " *c* has been directly verified ". To say that we have a reason *f* for *c* does imply that *c* is at least probable. It does not imply that *c* has already been directly tested. Now, if " correctly predict " is used in any ordinary sense, then the question " How can we now predict an event which is not yet ? " produces no cramps and can easily be answered by referring to the truth of past predictions in certain circumstances. Questions like " How can we now predict something which is not yet ? " give rise to headaches only if " *c* can now be correctly predicted " is used in such a way as to imply " *c* has been directly verified ". Sentences like this then produce cramps and headaches because they are not really questions at all. They are like rhetorical questions. The sentence " How can we now predict something which is not yet ? " is then another way of *asserting* the *necessary* proposition that in the high sense of " predict " in which " *c* can now be correctly predicted " implies " *c* has been directly verified ", it is impossible ever to predict a future event. But this of course does not at all contradict the commonsense view that in the ordinary sense of " predict " we can frequently predict future events. This objection, too, is therefore an *ignoratio elenchi* by redefinition.

To be more precise : the sentence " How can we now predict something which is not yet ? " produces a cramp if one believes oneself to be asking the (easy) question which the sentence expresses with every word in it used in its ordinary sense when one is in fact *asserting* the necessary proposition that in a certain high sense of " predict " we can never predict anything at all.

Following Moore, Mr. J. N. Findlay has forcibly drawn attention to the queerness of the philosopher's doubt when he utters

[1] This distinction is brought out very lucidly by Sydney Hook in his *John Dewey*, p. 79.

sentences like " But how can any one set of facts furnish a valid basis for an inference concerning another set of facts ? " [1], " How do you know that one thing ever happens after another ? " or " How do you know that one thing is ever to the left of another ? " Findlay suggests that we take a specific instance—*e.g.*, what we would normally describe as a pencil lying to the left of a pen—, point it out to the doubting philosopher, and say " This is how ".[2] In the case of predictions we could take a piece of chalk and call out " I now predict that when I release this piece of chalk it will move in a downward direction ". We would then release it, and, as it falls in a downward direction, we would point to it and say " This is how we can know in advance ". Since the philosopher is just as familiar with these facts as we are and since he does not, in one important sense at least, query any of them, it is apparent that, without realising it, he is using one or more of his words in a strange sense.

[1] Williams, *op. cit.*, p. 227.

[2] " Time : A Treatment of Some Puzzles ", *Australasian Journal of Psychology and Philosophy*, 1941, p. 217 ; *cf.* also Friedrich Waismann's introduction to Schlick's *Gesammelte Aufsätze*, pp. xxi ff.

II.—A DEFENCE OF 'SYNTHETIC NECESSARY TRUTH'.

By Stephen Toulmin.

I

There is a certain class of propositions (which, in order to avoid begging any questions, I shall refer to as 'Type-Q' propositions) about whose logical status contemporary philosophers have come to hold radically-opposed opinions, opinions separated by an apparently unbridgeable gulf. Typical of the class are the two propositions,

> "Seven and five make twelve" and
> "Nothing can be both red and green all over".[1]

Others which have at one time or another been suggested as belonging to the class are the propositions,

> "Such and such a type of intention or emotion would necessarily be fitting (or unfitting) to such and such a kind of situation",[2] and
>
> "One ought always so to choose that the same volition shall comprehend the maxims of one's choice as a universal law".[3]

It is generally agreed that the most typical propositions of this class are 'necessary', *i.e.*, that the accumulation of evidence from experiments or observations is neither required for, nor relevant to the establishment of their truth or falsity; but beyond this point there is no general agreement. Some philosophers hold that they are 'synthetic', *i.e.*, that their truth does not follow from the definitions of the terms involved alone, and that they are known by '*a priori* insight',[4] 'immediate apprehension',[5] or 'intuitive induction'.[6] (This opinion I shall refer to as the 'Synthetic Theory'.) Such philosophers find nothing obnoxious in the idea of propositions which are both 'synthetic' and 'necessary'. Their opponents, on the other hand, seem to regard the phrase 'synthetic necessary proposition' as a contradiction in terms; for they hold it to be demonstrable that there can be no such propositions, and *a fortiori* that 'type-Q' propositions are not both synthetic and necessary. Any necessary proposition which seems on a superficial examination to be also 'synthetic', they declare, will be found on closer inspection to be

' analytic '—true ' by definition ', or by ' linguistic convention '—after all :

> " Our knowledge that no observation can ever confute the proposition ' 7 + 5 = 12 ' depends simply on the fact that the symbolic expression ' 7 + 5 ' is synonymous with '12 ', just as our knowledge that every oculist is an eye-doctor depends on the fact that the symbol ' eye-doctor ' is synonymous with ' oculist '. And the same explanation holds good for every other *a priori* truth." [7]

(This opinion I shall call the ' Analytic Theory '.)

In this paper, I want to reconcile these opposing views. I shall argue that there is nothing self-contradictory in talking of ' synthetic necessary propositions '. With the help of an example more familiar in everyday life than in philosophical discussions, I shall try to throw fresh light on to the logical status of ' type-Q ' propositions. I shall point out that the issue has been given a delusive sharpness by the introduction of certain irrelevant ' epistemological ' arguments ; namely, attempts to answer the conundrum, " How do we ' know ' synthetic necessary truths ? ", as though it were like the question, " How do cats see in the dark ? " Leaving these aside, I shall attempt to reconstruct the source of the Analytic Theory's plausibility, and at the same time to discover why supporters of the Synthetic Theory are led to get so hot under the collar at the prospect of the Analytic Theory (" a doctrine which must encourage the widespread depreciation of reason in so far as it has any influence at all ".[8])

II

The Analytic Theory, or at any rate the most usually published form of it, as interpreted in the most natural and literal senses of the terms employed, has defects which supporters of the Synthetic Theory have pointed out ably and at length : these defects I intend only to summarise. To begin with, although it can be agreed that the truth of type-Q propositions " depends wholly on the meaning of the terms used " [9]—supporters of the Synthetic Theory neither need to, nor do dispute this—this is not the same as saying that they are true or false in virtue of the *definitions* of the terms used. Their truth certainly does not depend on anything which anyone but a philosopher with an axe to grind would call ' definitions ' or linguistic ' rules ' : books of grammar and dictionaries [10] do not suffice to establish typical type-Q propositions any more than they do matters of fact. Further, it is quite

certain that, whatever language a type-Q proposition were expressed in, it must always remain logically unaffected : it must be as true or as false in Chinese, Afghan or Malay as it is when expressed in languages we can understand.[11] This fact is concealed by those who claim that the truth of type-Q propositions is based on ' linguistic conventions ' ; and the use of this phrase also gives such propositions a misleading air of arbitrariness (' conventionality '), which supporters of the Synthetic Theory understandably find offensive.[12]

Now it seems to me that supporters of the Analytic Theory ought to grant that these are perfectly valid objections to their view, certainly in the forms in which it has been expressed up to now. Indeed, I suspect that they only hesitate to grant this because they believe that to do so must commit them to the Synthetic Theory *as a whole*. Such a belief is mistaken : they can quite well afford to allow that there is some value in Kant's division of necessary propositions into ' analytic ' and ' synthetic ', and even agree to say, for the purposes of argument, that there can be such things as ' synthetic necessary propositions ', without finding themselves forced to swallow, willy-nilly, all that seems to them unpalatable in the usual expositions of the Synthetic Theory. For what is pernicious in these expositions is, surely, not so much the decision to apply the title ' synthetic necessary proposition ' to certain propositions, as the doctrine that, granted the existence of ' synthetic necessary truths ', we must ' know ' them by ' intuition ', by ' *a priori* insight ', by a ' rational faculty of immediate apprehension ' (" Something accompanying and behind all the senses, [which] receives *some* new ideas by itself and without the medium of sensation "[13]). This doctrine I shall refer to as ' the Doctrine of the Inner Eye '.

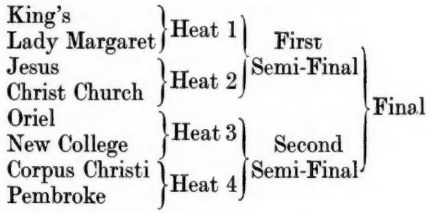
It is my first task to show that behind Kant's division of necessary propositions there lies an indisputable distinction, which is conveniently marked by the use of his terms, ' analytic ' and ' synthetic ' ; but that to grant this is not necessarily to grant the Doctrine of the Inner Eye.

III

I can best do this with the help of examples taken from an everyday activity, that of running a regatta, consisting of a number of ' knock-out ' competitions. Here (away from the blinding dust of the philosophical arena) we can recognise the distinction between ' synthetic ' and ' analytic necessaries ' without getting entangled in epistemological side-issues ; and we can

carry back the lessons learnt when we return and reconsider the more complicated and contentious ' type-Q ' propositions.

Suppose, then, that eight crews enter for the Visitors' Challenge Cup, and that the draw comes out as follows :—



I may now have occasion to say :

(*a*) " King's can't get into the final ",

(*b*) " King's can't get into the second semi-final ", or

(*c*) " King's and Lady Margaret can't both get into the final ".

Of these propositions, (*a*) is unquestionably ' empirical '. If called upon to justify my belief I shall appeal to past form as evidence, saying " Their stroke is too short ", " Their blade-work is ragged ", or " The other crews in the top half of the draw are too fast for them ". No such considerations are, on the other hand, relevant to the truth of (*b*) and (*c*).

There are, however, other grounds on which (*b*) and (*c*) may be called ' empirical ', for King's and Lady Margaret might very well have been drawn elsewhere. (By exchanging King's and any of the four bottom crews, you falsify both (*b*) and (*c*).) In order to discount this empirical element—what we call ' the luck of the draw '—let us consider instead the two propositions,

(*d*) " The first crew in the draw can't get into the second semi-final ", and

(*e*) " The first two crews in the draw can't both get into the final ".

These propositions, I suggest, can fairly be called both ' necessary ' and ' synthetic '.

In order to confirm that (*d*) and (*e*) are necessary, notice that no observation or experience could ever confute them. We do not need to go to more and more and more regattas in order to make sure of their truth : we know very well that they are true without that. If, for instance, a semi-final in which the first crew in the draw competed took place later in time than that between crews from the bottom half, that would not falsify (*d*). It would only show that the ' second semi-final ' had been rowed before the ' first semi-final '.

But, granted that the propositions (*d*) and (*e*) are ' necessary ', are they not perhaps also ' analytic ' ? I think it would be an unnatural extension of the term to call them ' analytic '. It would certainly be an unnatural extension of the terms ' definition ' and ' convention ' to say that they were true ' by definition ' or ' by linguistic convention '. Nothing which it would be at all natural to give as ' definitions ' of the terms ' crew ', ' draw ', ' heat ', ' final ', etc., (*e.g.*, the definitions in the *Oxford English Dictionary*) would be sufficient to establish the truth of (*d*) and (*e*). In protesting against the Analytic Theory on these grounds, supporters of the Synthetic Theory, such as Dr. Ewing,[14] seem to me to be entirely in the right. If it *is* from ' conventions ' or ' rules ' that the truth of (*d*) and (*e*) follows, it is not from ' linguistic ' rules or conventions : it is from *the rules for running a regatta* ; and these are something quite different from the definitions of ' crew ', ' draw ', ' heat ', ' final ', etc.

Advocates of the Analytic Theory may, of course, reply that to say this is only to make the truth of (*d*) and (*e*) follow, in addition, from the definitions of the terms ' knock-out competition ' and ' regatta ', and in a sense this is true ; but, at the same time, it both is true in a misleading way and concedes the crucial point. To explain this : it is conceivable that one might come across people engaged in an activity closely resembling that which we call ' running a regatta ', who denied the propositions (*d*) and (*e*). It might be that we found, for example, that among these people the race between the first two crews in the draw was taken as the ' decider ' (so falsifying (*e*) in a practical manner), the winner of this race being given the prize and treated as ' the champion crew '. And, if this were to happen, we should no doubt want to say (according to the circumstances) that what they were running was ' a misconducted regatta ', ' a different kind of regatta ', or even ' not a regatta at all ', certainly ' not what *we* call a regatta '. But suppose that considerations like these were held to show that the truth of the propositions (*d*) and (*e*) does, after all, depend on ' definitions ' or ' linguistic conventions ' (since their being true is part of what we should require before agreeing to apply the terms ' knock-out competition ' and ' regatta ' to any activity) ; then we must give a two-fold answer :—

(i) The ' definitions ' at issue are not those of the terms involved in (*d*) and (*e*) *alone*, for the words ' regatta ' and ' knock-out competition ' do not occur in either of these propositions. That fact alone, on the definition of ' synthetic ' adopted in this paper, is enough to justify calling them ' synthetic ' : the crucial point is therefore conceded.

(ii) Even granted that the truth of (*d*) and (*e*) follows from all these ' definitions ' taken together, the ' definition ' of ' regatta ' to which we must appeal is no arbitrary one. There is all the difference in the world between saying, " That's not what we call a ' regatta ' : the word for that is ' raffle ' ", (so asserting a purely linguistic matter) and saying, " That's not what we call a regatta ; that's hardly more than a raffle ! ", meaning, " That kind of competition is no test of the skill of the competitors : winning it is hardly more than a matter of pure chance, so it doesn't serve the purpose of a regatta at all ! " The distinction between the *usages* of the terms ' regatta ' and ' raffle ' may be a linguistic convention, but it is (to put it at the best) hopelessly misleading to talk of the difference in *purpose* between a raffle and a regatta either as ' conventional ' or as ' a matter of words '.

Let me sum up the lessons to be learnt from this example. There is an indisputable distinction to be drawn between necessary propositions of two kinds : those like " An oculist is an eye-doctor ", whose truth follows from the definitions of the terms involved alone, and those like " The first two crews in the draw can't both get into the final ", which cannot be proved true without a further appeal. This distinction is conveniently marked by calling propositions of the first kind ' analytic ' and those of the second ' synthetic '. (I shall argue in a moment that this use of the terms ' synthetic ' and ' analytic ' reflects Kant's own practice, if not perhaps his intentions, and that, in adopting it, we shall be doing him more justice than do the advocates of the Doctrine of the Inner Eye.) Those philosophers, such as Ayer, who have in the past supported a ' purely analytic ' theory of necessary propositions, need not be afraid of recognising this distinction ; for, in order to justify what on these standards we shall call ' synthetic necessary propositions ', we need not appeal to the evidence of any mysterious ' sixth sense ', ' insight ' or ' intuition ', but only to our understanding of the nature of the subject-matter under discussion, that is, to the ' logical type ' of the concepts involved. Still, the definitions of the terms involved are, by themselves, not enough. In order, for instance, to prove the truth of (*d*) and (*e*), we have to know not only the definitions of ' crew ', ' final ', ' draw ', etc. : we have also to know (and, it is important to note, in considerable detail) what it is to run a ' regatta ', as opposed to a ' sweepstake ', a ' championship competition ', etc. If we are to prove (*d*) and (*e*), " we must ", to use Kant's own words, " advance beyond the cognition of the objects to a critical examination of the subject ".[15]

IV

It is interesting to find Kant making a remark like this. His intentions are often obscure, and the way in which he uses terms like 'subject' is, as a result, consistently ambiguous. (Does he mean 'thinker' or 'subject-matter'?) If, however, we discount the mock-psychological façade, and examine the logical structure behind it, we shall find that his position is not widely different from ours.

Consider, for example, his 'principle of the autonomy of the will', the 'synthetical and necessary proposition' in elucidating which he makes the remark just quoted. This principle he states in the words,

> "Always so to choose that the same volition shall comprehend the maxims of our choice as a universal law."[16]

After stripping away the psychological trappings, I want to reinterpret it in the form,

> (*f*) "The only principles to which one ought to appeal in justification of one's decisions and actions are ones which apply universally—not to me rather than you, now rather than another time, etc."

(This may not be what Kant said, but it seems to me to have something very closely to do with what Kant said, and to be a proposition well worth discussing.)

Proposition (*f*) is, in my sense of the phrase, a 'synthetic necessary' proposition. We do not discover its truth by observation or experiment, as we go along: it is 'necessary' or nothing. At the same time, its truth could hardly be said to follow from anything one would naturally give as 'definitions' of the terms involved *alone*: it is, therefore, 'synthetic'. But, bearing in mind the lessons of the regatta example, it is not hard to discover what else it is that the truth of (*f*) *does* depend on. For, just as in the case of (*d*) and (*e*) it is taken for granted that we are talking about a 'knock-out competition' and a 'regatta', so (*f*) has to be understood as referring to the 'moral' justification of our decisions and actions. It is true that, if people stopped talking about what they ought 'morally speaking' to do, that is, if they ceased to take 'moral' considerations into account in making up their minds, the truth of (*f*) might be forgotten or denied. But, if that were to happen, we should have to say that they were no longer talking about the same kind of thing as before; that it was appeals to 'privilege', to 'expediency', to 'authority', with which they were concerned, rather than appeals to 'morality'; and this would make nonsense of (*f*), rather than

falsifying it, since, in the absence of any qualification, we understand 'ought' as meaning 'ought (morally speaking)'.

The truth of (*f*) depends, therefore, not only on the definitions of the terms involved, but in addition on what we mean by 'morality', as opposed to 'privilege', 'expediency', etc. Appealing to universal principles is a part of what we mean by 'appealing to morality', and by 'speaking morally'. Once again, we cannot question the truth of the synthetic necessary proposition (*f*), so long as we understand and accept the nature of the subject-matter under discussion; namely, that it is a question of 'morality'. And, once again, to claim that this proposition is true 'by linguistic convention', or 'by definition', after all (even if it does not actually follow from the definitions of the terms involved alone) is hopelessly misleading: the distinction we draw between 'morality' and 'privilege' is in most situations more than 'a matter of words'.

Before leaving Kant, may I interject a few remarks about the *Critique of Pure Reason*? When Kant declares that we cannot help thinking in terms of spatial, temporal and causal notions, he seems to be concerning himself with a kind of super-empirical psychology; and this raises endless problems. But there is logical backing for what he says, as we can recognise if we recall his preoccupation with the question, "How is knowledge possible?"[17]

Consider the proposition,

(*g*) "One cannot help thinking in terms of spatial, temporal and causal notions".

As it is expressed, this is certainly no analytic truism; but, if we add the clause, "If it's knowledge one is after", we can trace the source of its necessary truth. If you forswore the use of all spatial, temporal and causal notions, nothing which you said would be very informative, or would communicate anything which could properly be called 'knowledge'. It is true that you might still be able to say, "Something happened to me somewhere sometime". Yet even this might have to be ruled out, since the words 'somewhere' and 'sometime' could only be used correctly by one who understood words like 'here' and 'now'; in which case you could only exclaim, "Man!", or "Ow!", or "Hurrah!". Now, supposing that this *were* all that you could say to me, I should be entitled to reply, "That's not 'knowledge'!" By this I should mean, not "The word for that is 'rumour' or 'hokum' or 'exclamation', not 'knowledge'", but rather, "That gives me none of the kind of help needed in this situation, such as you would have given me by saying, 'A tall man in a

mackintosh jumped out from behind the gate as I was walking past it five minutes ago and struck me on the head, breaking my spectacles'." One might, therefore, argue very plausibly that proposition (*g*) is both true, and necessarily true, though its proof requires us to appeal not only to the definitions of the terms involved, but also to the nature of the subject at issue ; that is, to the distinction between what can and what cannot properly be called ' knowledge '.

V

It should now be possible to explain both the source of the Analytic Theory's plausibility, and the horror which it so often arouses in opponents.

First, then, what is it that has led philosophers to advocate the Analytic Theory, despite the comparatively obvious fallacies involved ? Recall the lessons of the examples we have examined. There is a slightly eccentric sense of ' follow ' such that type-Q propositions may be said to follow from certain ' definitions ', or rather from the nature of certain distinctions which we are in the habit of making (and for very good reasons) ; such as that between a ' regatta ' and a ' raffle ', that between ' morality ' and ' privilege ', and that between what can and what cannot properly be called ' knowledge '. (These distinctions, however, define not the terms but the logical type, or the nature of the subject-matter of the type-Q propositions concerned.) In consequence, there is another class of propositions with which type-Q propositions may easily be confused. This class is made up as follows : for every type-Q proposition, we may construct a corresponding proposition in which the nature of the subject-matter is made explicit. In place of

(*e*) " The first two crews in the draw can't both get into the final ",

we may consider

(*e'*) " In a ' knock-out competition ' in a ' regatta ', the first two crews in the ' draw ' can't both get into the ' final ' ".

In place of

(*f*) " The only principles to which one ought to appeal are universal ones . . .",

we may consider

(*f'*) " When taking ' moral ' considerations into account, the only ' principles ' to which we ' ought ' to appeal are universal ones . . ."

In place of

(*g*) " One cannot help thinking in terms of spatial, temporal and causal notions ",

we may consider

(*g'*) " If one is to obtain ' knowledge ', one cannot help thinking in terms of spatial, temporal and causal notions ".

And likewise, in place of

(*h*) " Seven and five make twelve ",

we may consider

(*h'*) " In ' the arithmetic of natural numbers ', seven and five make twelve ".

It is reasonable enough to call the propositions (*e'*) to (*h'*) ' analytic necessaries '. And, since these propositions only make explicit what is already implicit in the type-Q propositions (*e*) to (*h*), the advocates of the Analytic Theory may have argued that (*e*) to (*h*) must therefore also be classed as ' analytic necessaries '. " In any case ", they may say, " we know nowadays how much more complicated definitions may be than used to be suspected : it is therefore pardonable to confuse, or rather to ignore the differences between the two classes of proposition." But, whatever may be said in their defence, and however much one may sympathise with them in their distaste for the Doctrine of the Inner Eye, the differences between the propositions (*e*) to (*h*) and the propositions (*e'*) to (*h'*) remain ; and it remains an extension of the notion of a ' definition ' far beyond its everyday limits to pack into it everything needed to prove the truth of type-Q propositions from the ' definitions ' of their terms alone, for this means including in every ' definition ' all those things which distinguish a notion of one type from notions of *other* logical types as well as everything marking it off from other notions of the *same* type, all those things which make red a ' colour ' as well as everything marking it off from green, blue, brown, etc.

So much for the fallacies behind the Analytic Theory. But what makes it seem to so many of its opponents not just false but outrageous ? One of the lessons of the regatta example was the difference between two similar-looking classes of propositions :—

(i) " That's not a ' regatta ' ; that's a ' raffle ' ".
" That's not an ' oculist ' : that's an ' optician ' ".

(The issue in these cases is purely ' linguistic '. The proposition is intended to correct someone's usage, and the second half can in each case be read as " the word for that is ' raffle ', ' optician ', etc.".)

(ii) " That's not a regatta : that's hardly more than a raffle ! "

" That's not an appeal to morality : that's an appeal to privilege ! "

" You can't call that knowledge ! "

" That's a funny kind of arithmetic ! "

(The issue in these cases is far from being purely linguistic. The proposition is intended not just to correct someone's usage, but to make him think and behave very differently. The issue can, therefore, hardly be called either ' conventional ' or ' a matter of words '.)

Quite apart from confusing the propositions (*e*) to (*h*) and (*e'*) to (*h'*), advocates of the Analytic Theory have to assimilate all the members of class (ii) to class (i). Only by treating the members of class (ii) as ' matters of words ', like those in class (i), can they justify themselves in calling type-Q propositions ' analytic ', true ' by definition ', true ' by linguistic convention '. And to do this is clearly to misrepresent both the nature of class (ii) propositions, and that of the type-Q propositions dependent on them. In view of this, we need hardly be surprised at the feelings of outrage which supporters of the Synthetic Theory express when faced with the Analytic Theory. (Even so, we may feel that they are hardly justified in suggesting that belief in the truth of the Analytic Theory should lead one to abandon discussion in favour of violence as a means of settling disputes ! [18])

VI

A few remarks, in conclusion, about the Doctrine of the Inner Eye, since this is the real bone of contention between supporters of the two theories. It should be clear by now that the existence of ' synthetic necessary ' propositions and the ' synthetic ' nature of type-Q propositions are questions wholly independent of this doctrine. There is not much temptation to invoke any ' intuition ' or other ' rational faculty ' to justify one's certainty of the truth of the proposition (*e*), " The first two crews in the draw can't both get into the final " : it is clear, in this case, that a knowledge of what it is ' to run a regatta ' is all that we need to appeal to. The question we now have to ask is why this independence should be any less obvious in the case of more typical type-Q propositions. How is it, then, that so many philosophers have come to think that some special ' faculty ', ' insight ' or ' intuition ' was needed to inform us of the truth of type-Q propositions ?

I can, at the moment, suggest only the beginnings of an answer

to this question. Notice to begin with two kinds of proposition of which it is possible to be ' absolutely certain '. On the one hand, (1) there are those propositions (for example, " The ace of hearts is in my hand ") of whose truth we are certain without there being any question of our adducing ' reasons ' : if challenged, we can only appeal to ' the evidence of our senses '. On the other hand, (2) there are those propositions whose truth we cannot question if we understand the nature of the subject-matter at all. Type-Q (synthetic necessary) propositions belong, as I have been trying to explain, to the second class : advocates of the Doctrine of the Inner Eye talk as though they belonged to the first.

Consider the following passage written by a supporter of the Synthetic Theory (incidentally, by one who claims[19] not to ' postulate some mysterious faculty ' to explain our knowledge of synthetic necessary truths) :—

> " It is surely plain that some *a priori* propositions, *e.g.* everything which has shape has size, a thing cannot be both red and green, if one thing is above another and the second is above a third the first is above the third, all three-sided rectilinear figures have three angles, could be seen to be true without the use of language. A person who was capable of forming visual images might quite well see the truth of any of these propositions without having to put them into words, and therefore their truth cannot possibly depend on the structure of language." [20]

The crucial and ensnaring phrases in this passage are ' seen to be true ' and ' see the truth of '. Certainly, when talking about propositions of class (1), we are accustomed to using phrases of the form ' seeing the ace of hearts in my hand ', ' seeing that the ace of hearts is in my hand ', ' seeing that the proposition " The ace of hearts is in my hand " is true ' and ' seeing the truth of the proposition " The ace of hearts is in my hand " ', interchangeably. But this practice can be extended to propositions of class (2) only through a misunderstanding. If this *is* done, however, the Doctrine of the Inner Eye acquires considerable plausibility. For then, having noticed that seeing the ace of hearts in your hand is much the same as seeing the truth of the proposition, " The ace of hearts is in my hand ", you may all too easily begin looking for the experience whose description is interchangeable with the phrase ' seeing the truth of the proposition, " Nothing can be both red and green all over " '.

Once begun, this search need never end, for no answer can be at all satisfactory. No experience will do for the propositions, " Nothing can be both red and green all over " and " The first

two crews in the draw can't both get into the final ", what seeing the ace of hearts in your hand does for the proposition, " The ace of hearts is in my hand ". " Use your eyes ", is a fair answer to the question, " How am I to know that the ace of hearts is in my hand ? " : the answers to the questions, " How am I to know that the first two crews can't both get into the final ? " and " How am I to know that nothing can be both red and green all over ? " are to be given, not by any appeal to a ' sixth sense ' or to ' visual imagery ', but by reference to the way in which regattas are run, the purpose of regattas, etc., and to the nature of colour-concepts, the purpose of classification by colours, etc. One can conjure up a visual image of the ace of hearts in one's hand, and to do this is (if you like to call it that) ' to see what is meant by the words " The ace of hearts is in my hand " '. But, conjure up what visual images you choose, you will never find one which shows you ' what is meant by the words " Nothing can be both red and green all over " ' : all you can imagine is *red and green objects*. Nor is visualising three dots, one above the other, in any sense the same as ' seeing the truth of the proposition, " If one thing is above another, and the second is above a third, the first is above the third " ' : the most such a visual image could ever be said to do would be to *remind* you or to *convince* you of the truth of that proposition.

To resort to ' intuition ', ' rational faculties ', ' visual images ' and so on is to overlook or forget the fact that ' seeing the truth of a type-Q proposition ' is not a matter of ' having the right experiences ', as it would be if type-Q propositions belonged to class (1), but is a matter of ' understanding the nature of the subject-matter '. And understanding the nature of the subject-matter is (to adapt Professor Ryle's illuminating distinction [21]) as much ' knowing how. . . .' or ' knowing what it is to. . . .' as it is ' knowing that. . . .' The natural thing, for example, to say about a man who knows that seven and five make twelve is that (unlike some) he knows ' how to count ' ; not that he has access, which others are denied, to certain mysteriously ultimate and ineluctable facts about the universe (or what Dr. Ewing inscrutably calls ' the real ' [22]). Likewise, the man who knows that p and q together entail r is a man who knows ' how to make inferences '.

What we commonly call ' insight ' may help mathematicians like Fermat and Ramanujan and logicians like Russell to get their answers more quickly than other people, and even to get answers which they would not have reached without it ; but it is no substitute for a proof. ' *A priori* insight ', again, might *help*

one to see the truth of complicated type-Q propositions; but it is no substitute for a discursive analysis of the types of situation and activity in connexion with which the propositions are used, and the functions they perform in those situations. Intuition, visual imagery, insight and what-you-like may be short cuts to confidence in the truth of synthetic necessary propositions, but they cannot constitute a proof.

REFERENCES:

[1] See A. C. Ewing on "The Linguistic Theory of *A Priori* Propositions", *Proc. Aristot. Soc.*, 1939-40, pp. 207-244: I shall refer to this paper as 'E'.
[2] C. D. Broad, *Five Types of Ethical Theory*, p. 282.
[3] Immanuel Kant, *Fundamental Principles of the Metaphysics of Morals*, tr. Abbott, p. 71.
[4] See, for example, E, p. 213.
[5] See, for example, D. D. Raphael, *The Moral Sense*, p. 34.
[6] Broad, *loc. cit.*, ; Raphael, *op. cit.*, p. 193, etc.
[7] A. J. Ayer, *Language, Truth and Logic*, 1st ed., p. 115.
[8] E, p. 209.
[9] *Cf.*, E, pp. 231-232.
[10] E, p. 213.
[11] E, pp. 217-219.
[12] E, pp. 209, 232, 240-241.
[13] Raphael, pp. 2-3, 34, etc.
[14] *Cf.*, the paper (E) quoted above.
[15] *Loc. cit.*
[16] *Loc. cit.*
[17] This was pointed out to me by Professor von Wright in discussion.
[18] See E, pp. 240-241.
[19] E, p. 243.
[20] E, p. 217.
[21] See G. Ryle, "Knowing How and Knowing That", *Proc. Aristot. Soc.*, 1945-46, pp. 1-16.
[22] E, p. 211.

III.—DETERMINABLES, DETERMINATES AND DETERMINANTS (II).

By Arthur N. Prior.

(iv)

Johnson is inclined (I am not sure with what historical justification) to identify his "structural" propositions with those which Kant calls "analytic". At the same time he is at some pains to distinguish them from those which Mill calls "verbal". "The structural proposition", he contends, "does not depend upon any arbitrary assignment of meaning to a word. . . . For instance, taking as subject-term 'the author of the *Republic*', then 'The author of the *Republic* wrote something', would be verbal, while 'The author of the *Republic* is an individual' would be structural. In reality the subject of a verbal proposition, and the subject of a structural proposition are not the same; the one has for its subject the *phrase* 'the author of the *Republic*', and the other the *object* denoted by the phrase. This is the true and final principle for distinguishing a structural (as well as a genuinely real or synthetic statement) from a verbal statement." But is it really as clear as all that, that "The author of the *Republic* wrote something" is about the phrase "the author of the *Republic*"? And while the structural statement is fairly plainly not about this phrase, could it not be about the word "individual"? And as for structural propositions not depending on the assignment of meaning to a word, is there any proposition that does not depend upon that?

In his explicit discussion of the verbal proposition (I. iv. 8), Johnson says that "as verbal, it cannot strictly be said to be either true or false, because it does not declare a fact, but rather expresses an intention, a command, a request. The technical scientist puts forward his definitions in this spirit, when he asks readers to allow him to use a term with a certain signification which is explained by his definition. Thus a verbal proposition is neither true nor false, because it is properly expressed, not in the indicative, but in the imperative or other similar mood." That verbal propositions "are not, strictly speaking, susceptible of truth or falsity" is, of course, the doctrine of Mill (*System of*

Logic, I. vi. 1), and the reason which Johnson gives for their peculiarity in this respect seems to be the true one. But when we say "The author of the *Republic* wrote something", can it be seriously maintained that what we are expressing is a proposal to use the subject-phrase in a certain way? Johnson adds, indeed, that "if by a verbal proposition is meant one that assigns the meaning of a word as conventionally used, . . . then, inasmuch as the proposition asserts that such or such *is* the convention, it may be true or false". But is even this—not the suggestion of, but an assertion about, a verbal convention—what we are putting forward when we say "The author of the *Republic* wrote something?" It seems to me that what gives "The author of the *Republic* wrote something" its peculiar character is precisely the fact that a certain convention—that we will not call anyone an "author" unless he has written something—has been already agreed to before the proposition is framed. This is still clearer in the case of an example given by Johnson in another connexion—"An integer between 3 and 4 is greater than 3 and less than 4." What he says of this proposition is not that it is incapable of truth or falsehood, but that it is quite definitely true, despite the fact that its subject is non-existent—the illustration is brought up in connexion with his doctrine that "*S* is" means "*S* denotes something of which some adjective may be truly predicated", which he has to qualify by adding "in a proposition not merely verbal" (I. v. 3).

Johnson's reference to the structural proposition's "not depending upon the arbitrary assignment of meaning to a word" is possibly an echo of Dr. J. N. Keynes's *Formal Logic*. In discussing Mill's distinction between real and verbal propositions, Dr. Keynes distinguishes from both of them a type which he calls "formal". These are "valid whatever may be the meaning of the terms involved"; examples are *If all A is B then no not-B is A*, *An animal is an animal* (p. 52). They are, in fact, just the "tautologies" with which we are nowadays familiar. Verbal propositions differ from these in so far as their "validity" *does* depend on "the meaning of the terms involved". Dr. Keynes includes definitions among verbal propositions (p. 50), but only definitions of the form *X means a b c*, not definitions of the form *The meaning commonly attached to the term X is a b c* (p. 49*n*). For definitions of the latter type may be true or false, and Dr. Keynes, like Johnson, agrees with Mill that this distinction is inapplicable to verbal propositions.

Now it seems to me that the propositions which Mill, Keynes and Johnson all agree in calling verbal, are of two quite distinct

types. There are, in the first place, definitions, in the sense of mere proposals as to how a word shall be used; and these are incapable of truth or falsehood. And then there are propositions which are seen to be formal as soon as their terms are defined (by an agreed-upon definition). For example, "An integer between 3 and 4 is greater than 3 and less than 4" is seen to be a formal proposition as soon as we give its translation, "An integer greater than 3 and less than 4 is greater than 3 and less than 4". This sort of verbal proposition is quite definitely true, though tautological. Its truth is secured, not by the definition of its terms alone, but by its form, once its terms are defined. And Johnson's real problem is to distinguish the formal feature which gives such certainty to "An integer greater than 3 and less than 4 is greater than 3 and less than 4", or "The man who wrote something called the *Republic*, wrote something", from that which gives its certainty to "The author of the *Republic* is an individual". It is, in fact, just our problem of distinguishing structural propositions from tautologies.

Turning to the structural proposition first, I am not sure that "The author of the *Republic* is an individual" is as happy an example as Johnson could have chosen, or as he himself gives us in other connexions. His general formula for such propositions is "The given thing is *MP*", where *M* and *P* are the determinables by which the predicated category is defined; and two of his other examples are "Red is an adjective" and "That *matter exists* is a proposition", where in each case "the given thing" is referred to by what amounts to a proper name. The trouble with "The author of the *Republic* is an individual" is that more is said of the individual in question than that he is an individual, namely that he wrote the *Republic*. It is not that "The author of the *Republic* is an individual" is not a structural proposition; but it is something more as well. "Plato is an individual" would probably be a more "pure" example. And when we have the example in this form, it is plain that the subject must exist, and that in the special sense in which individuals do, if the proposition is to be true (as redness must exist, in the sense in which adjectives do—it must "subsist", that is—if "Red is an adjective" is to be true; and "That *matter exists*" must exist, in the sense in which propositions exist, if "That *matter exists* is a proposition" is to be true). In fact, what is peculiar about "Plato is an individual", by comparison with ordinary synthetic propositions, is that the existence of its subject is *all* that is necessary for it to be true. Its peculiarity is that of such a sentence as "This exists", where the word "this" is purely

denotative. Many modern logicians deny that " This exists " is a genuine proposition, and it is certainly empty enough ; yet we might say that if it were not true, and if " This " therefore did not denote anything, such propositions as " This is red " would be not merely false but meaningless. " Plato is an individual, stands in just this relation to " Plato wrote the *Republic* " ; it is in fact, the form of " This exists " which is presupposed in the significance of statements about authors.

On the other hand, " The man who wrote something called the *Republic* wrote something " does not at all depend for its truth on the existence of its subject, and if we interpret the definite article as an implicit assertion of the subject's existence, the proposition is not formal (and the original one, " The author of the *Republic* wrote something ", not " verbal "). This is only an example of a formal proposition if we take it to mean, " If anyone wrote something called the *Republic*, he wrote something ". This might also be expressed negatively, if a little awkwardly, as " It cannot be both true that a man wrote something called the *Republic*, and false that he wrote something ". Is seems essential to formal propositions that, even if their given verbal form is not compound, they should contain descriptive phrases which enable us to interpret them so ; and they are always reducible to the same basic sort of compound proposition—a denial that some conjunction of propositions is permissible. They are rules of consistency, reminding us that if we put certain statements together we " contradict ourselves "—that is, we take back with one statement what we have said with another, and so say nothing at all.

Another contrast between formal and structural propositions is in the range of other propositions whose significance depends upon their truth. If it were not true that whoever wrote something called the *Republic* wrote something, then a man might write something called the *Republic* and yet not write anything. And this would mean that the proposition " He did not write anything " could lose its power to refute " He wrote something " if we added to the latter, " but it was only the *Republic* ". But no proposition can perform its function of asserting something definite, a function essentially involving denial, if what it denies can as it were " escape " by getting conjoined with something else. If this part of a proposition's definiteness can be taken away from it in any one case, then we just cannot be sure that any proposition means anything. It is the significance of *all* propositions which is at stake when the truth of formal propositions is called into question. As the symbolic logicians say, a tautology is entailed by *any* proposition. But deny that Plato is an individual,

and only a limited group of propositions lose their meaning. In this respect the two types of "superfluous" propositions seem to resemble, and at least partly to coincide with, the two types of "extrinsic principles" dealt with by William of Ockham. By an "extrinsic principle" Ockham means a proposition presupposed by a demonstration though not entering into it as a premiss, and of these some, such as the Laws of Thought, are "presupposed, implicitly or explicitly, by all discursive thought and by all argument, and are such that their truth is recognised precisely in so far as any proposition is understood to be true or false". Others "are presupposed, not by all demonstrations, but only by those to which they apply" (Moody, p. 225. I think an example of what Ockham would call an "extrinsic principle" of the second sort would be, not "Plato is an individual", but "Individuals exist").

The range of relevance of formal propositions is, however, limited in another way. They are presupposed in the significance of all propositions, simple as well as compound; but they take cognisance only of relations between propositions as complete units capable of truth and falsehood—they do not bear upon the relations of substantives and adjectives *within* the proposition, which is the matter to which structural propositions are relevant (Plato must be an individual if the *characterising* of him as "author of the *Republic*" is to have significance). Formal or tautological propositions do, indeed, seem sometimes to be concerned with relations between the parts of "simple" propositions when these have either complex predicates or complex subjects. The Leibnizian commutative law, AB = BA, was originally applied to terms rather than to propositions, and a great deal of Formal Logic is occupied with the internal structure of "class" propositions. But this is only because apparently simple propositions with complex terms are in reality abbreviated expressions for compound ones—the apparent relations between their terms are reducible to relations between the other propositions of which they are composed. For example, "S is PQ" means "S is P and S is Q"; "All S is P" means "If this is S it is P, and if that is S it is P, and if that other is S it is P, etc.", running through all the subjects in the universe of discourse. It is one of Johnson's distinctions in the field of purely formal logic to have emphasised this—particularly in his articles on *The Logical Calculus*, but also in his *Logic* (II. iii. 8). And the relations between propositions which Formal Logic considers are relations like being "not jointly true".

Now this limitation of the subject-matter of formal logic

suggests that it may be a mistake to regard structural propositions as having no place in formal logic, or formal ones as having no place in the "logic of determinables". For formal propositions themselves seem to presuppose a limited universe of discourse—the universe in which the ultimate subjects of predication are propositions, and the predicates such terms as "true", "false", "jointly true" and so on. And on the other hand, Johnson's examples of structural propositions include one—"That *matter exists* is a proposition"—which is of a sort that may well have some special part to play in the formal logician's universe. May it not even be that to say that the truth of a tautology is presupposed in the significance of any proposition (*i.e.*, is implied both by its truth and by its falsehood), is just to say that the significance of any proposition presupposes that it is a proposition?

(v)

The structural proposition first comes up for consideration in Johnson in his account of the nature of syllogistic reasoning (*Logic*, II, i. 4-8). His actual analysis of the syllogistic form is remarkably anticipated in the *Port-Royal Logic*, Part III, Ch. XIII, though there is no parallel there to his statement of the principles underlying it. It is, indeed, instructive to see how the Port-Royalists go astray on points of formal detail just through their failure to have quite grasped what these principles are.

The Port-Royalists begin by pointing out that although a "perfect syllogism" must have two premisses, there is a certain sort of conclusion which may always be drawn from one premiss alone. This does not mean that they interpret the major premiss as an enumeration of statements of which the conclusion is one—*e.g.*, "All men are mortal" as "Socrates is a mortal man, Plato is a mortal man, etc.", so that no minor is required in order to demonstrate the mortality of Socrates. The conclusion which they draw from the single premiss is not identical with the conclusion of the completed syllogism, but one which is, as they say, "preparatory" to it—it is a conditional conclusion, affirming that the absolute conclusion is true if the other premiss is, thus:

> Every body which reflects the light from all its parts is uneven;
> Therefore, if the moon reflects the light from all its parts, it is an uneven body.

When the other premiss is granted, the "absolute" conclusion follows by a *ponendo ponens*, the whole taking the form

Every body which reflects the light from all its parts is uneven;
Therefore if the moon reflects the light from all its parts, it is an uneven body.
But the moon reflects the light from all its parts;
Therefore the moon is an uneven body.

With a different second premiss, a different conclusion would be obtainable by a *tollendo tollens*, thus:

Every body which reflects the light from all its parts is uneven;
Therefore if the moon reflects the light from all its parts, it is an uneven body.
But the moon is not an uneven body;
Therefore it does not reflect the light from all its parts.

Where a *ponendo ponens* is used in the second step, the syllogism falls into the first figure; where a *tollendo tollens*, into the second; so that this method of analysis well displays both what these two figures have in common and how they differ.

This last generalisation is not, however, made by the Port-Royalists, as it only holds good under certain conditions which they did not observe. It does not hold good, for example, when the principle of the *tollendo tollens* has already been implicitly used in the immediate inference to the "conditional" conclusion, as when we argue immediately from "Every body which reflects the light from all its parts is uneven" to "If the moon is not uneven it does not reflect the light from all its parts". (A *tollendo tollens* on the basis of this would, of course, yield a syllogism in Figure 1; and a *ponendo ponens*, one in Figure 2). The Port-Royalists give a number of examples of this sort; but one cannot help feeling that in such cases the inference of the "conditional" is indirect, and is more accurately displayed as two immediate inferences, thus:

Every body which reflects the light from all its parts is uneven.
Therefore, if the moon reflects the light from all its parts it is an uneven body.
Therefore, if the moon is not an uneven body it does not reflect the light from all its parts.

The Port-Royalists also give examples in which the premiss first

used is that which would be, in the first or second figure, the minor—examples analogous to

> The moon reflects the light from all its parts.
> Therefore, if every body which reflects the light from all its parts is uneven, the moon is an uneven body.

Here again, one feels, the reasoning is indirect, and might be better expanded to

> The moon reflects the light from all its parts.
> Therefore, if the moon is not an uneven body, not every body which reflects the light from all its parts is uneven.
> Therefore, if every body which reflects the light from all its parts is uneven, the moon is uneven.

If such a premiss is to be used in the first step of a " two-step syllogism ", it might most naturally appear in one in Figure 3, which would take some such form as this :

> The moon reflects the light from all its parts ;
> And the moon is not an uneven body.
> Therefore the moon is not an uneven body even if it reflects the light from all its parts.
> Therefore not every body is uneven which reflects the light from all its parts.

Here all is inverted—as, in the third figure, it must be ; that figure being the " inductive " figure, in which we do not either positively or negatively *apply* a given universal rule, but *disprove* one by means of a particular case incompatible with it. The immediate inference is now the second one ; and the " hypothetical " inference which precedes it is neither the *ponendo ponens* nor the *tollendo tollens*, but the unnamed one which Dr. Keynes mentions as " falling into line " with the third figure as the others do with the first and second (*Formal Logic*, 4th ed., p. 353)—the inference from " P, but not Q " to " P does not imply Q ".

As the Port-Royalists knew of no such third form of mixed hypothetical inference, their work contains no such examples as the one last given. Where the illustrations which they do give do not seem to demand intermediary steps of the kinds which I have indicated, it is evident that the power of their single premiss to yield its " conditional conclusion " is derived from the fact that it is itself, at bottom, conditional. The first inference mentioned, for example, might be represented thus :

> Anything is an uneven body if it reflects the light from all its parts:
> Therefore the moon is an uneven body if it reflects the light from all its parts.

But while both premiss and conclusion here are hypothetical, the inference itself is not a hypothetical inference: it prepares the way for a hypothetical inference, but is itself an inference employing what Johnson calls the *Applicative Principle*, according to which, "From a predication about 'every' we may formally infer the same predication about 'any given'." This principle may be applied, of course, to other "predications" about "every" and "any given" beside "conditional" ones. For example, we may immediately infer from "Every proposition can be subjected to logical criticism", that "That *matter exists* can be subjected to logical criticism"; or from "Every individual is self-identical", that "The author of the *Republic* is self-identical". But where the predication is conditional, we require not only the Applicative Principle, but also the *Implicative* Principle—the principle of the *ponendo ponens*, "From the compound proposition 'x and "x implies y' we may formally infer 'y'"—in order to arrive at a categorical conclusion. (As Johnson's analysis is only carried out for Figure 1, only the principle of the *ponendo ponens* is mentioned).

Having made this distinction, Johnson considers a possible objection that the applicative principle alone is insufficient to enable us to infer "The author of the *Republic* is self-identical" from "Every individual is self-identical", since the premiss here also makes a merely conditional predication, and all we can infer from it by the applicative principle is that "The author of the *Republic* is self-identical *if* he is an individual"; we need the additional premiss "The author of the *Republic* is an individual" before we can finally conclude (employing the implicative principle) that he is self-identical. It is at this point that Johnson claims that such propositions as "The author of the *Republic* is an individual" are "superfluous". He points out, in effect, that if we interpret "Every individual is self-identical" as meaning "Everything is self-identical if it is an individual", then we might just as well interpret "Everything is self-identical if it is an individual", as meaning "Every subject is self-identical-if-individual, provided that it is a *thing*": and so on *ad infinitum*. The interpretation of universal propositions as conditionals can only be carried a certain limited distance: and Johnson insists on stopping at the point where the subject-term places its object

in a category, *i.e.*, presents it to us as determinable in certain respects, but as not yet determined in any. In the example of the Port-Royalists, what is conditional is fully expressed when we render the major " Every *body* is uneven if it reflects the light from all its parts " ; it is merely misleading to express it as " Everything, if it is a body, is uneven if it reflects the light from all its parts ", and to suggest that we must be given " The moon is a body " as well as " The moon reflects the light from all its parts ", and employ the Implicative Principle twice over, before we can conclude that the moon is uneven. " The moon is a body " is not a necessary minor premiss, but what Johnson calls a superfluous " sub-minor ".

Johnson now points out (II. i. 10) that there is another species of " superfluous " premiss which one might be tempted to insert—one which he calls a " super-major ", the addition of which would involve a double use of the applicative principle. This would be the principle of the syllogism itself, that " What can be predicated of every member of a class, to which a given object is known to belong can be predicated of that object " ; from which the applicative principle would enable us to infer that if " square-stalked " can be predicated of every labiate, and the dead-nettle is known to be a labiate, " square-stalked " can be predicated of the dead-nettle. Johnson argues that the assertion that the validity of the inference depends on this interpolation of the *dictum de omni* as a premiss " would entail a contradiction ; because the principle claims that the two premisses are *alone* sufficient to justify the conclusion ". This suggested enlargement of the syllogism, in short, ignores the distinction between the premisses from which, and the principles in accordance with which, we reason ; or as Ockham would say, between " intrinsic " and " extrinsic " *principia*. The same point has been made, of course, by many writers, *e.g.*, Lewis Carroll in *What the Tortoise said to Achilles*.

In this last example of Johnson's, the syllogism is given in its ordinary unanalysed form. If the objection were brought forward in connexion with the syllogism as analysed it might take the form of an assertion that we cannot, at the second stage, employ the implicative principle without first stating it in general terms, and then using the applicative principle to demonstrate its relevance to this particular case. Having arrived, say, at the " conditional " conclusion that the moon is uneven if it reflects the light from all its parts, and it being granted that the moon does reflect the light from all its parts, a man might argue that we cannot conclude that the moon is uneven until it has been granted that

> " If any proposition is not true without a second being true also, and the first is, then the second is ",

and demonstrated from this, by the applicative principle, that

> " If ' the moon reflects the light, etc.' is not true without ' the moon is uneven ' being true, and ' the moon reflects the light, etc.' is true, then so is ' the moon is uneven '."

The contention that this interpolation is necessary is, of course, fallacious for the reason stated by Johnson in connexion with his own example. But what is now perhaps a little plainer is that, while Johnson's " superfluous sub-minors " are structural propositions, his " superfluous super-majors " are formal propositions. (This is so, at least, where the argument being thus embroidered is syllogistic. I should not like to attempt to give to the special " super-major " which he also provides for induction the appearance of a formal proposition ; here his whole argument, which is substantially Mill's, seems less plausible). We are here, therefore, at a point where a very close relation is visible between the two kinds of " extrinsic " or " superfluous " propositions—though without either of them actually entering into it, they underlie syllogistic argument in complementary ways.

In Johnson, the demand for a (structural) " sub-minor " to the syllogism, and the demand for a (formal) " super-major ", are considered separately, though in close succession. But it is conceivable that they might be made together. Having demanded a special employment of the applicative principle in order to bring the implicative one to bear upon the given case, an objector might say that even this employment of it is impossible unless he is also granted an appropriate " structural proposition " to help it out. Apart from his innumerable other demands, such a man would consider it necessary, before proceeding with the given syllogism, to go through the motions of the following piece of reasoning :

> If any proposition is not true without a second being true also, and the first is, then the second is.
>
> " The moon reflects the light, etc." and " The moon is uneven " are propositions.
>
> Therefore, if " The moon reflects the light, etc." is not true without " The moon is uneven " being true, and " The moon reflects the light, etc." is true, then so is " The moon is uneven ".

Considered apart from the absurd purpose for which we have imagined it to be brought forward, this argument is a simple, and valid, syllogism—it says that to every pair of propositions a certain rule applies, that certain given items constitute a pair of propositions, and that therefore the rule applies to those items. What is instructive about it is that the major premiss is a formal proposition, and the minor a structural. The conclusion would generally be classed as formal—perhaps, indeed, rather more confidently than the major premiss, which might be called a formal rule or principle rather than a formally true proposition. But the syllogism makes clear that there is a certain difference between the two, and that the more particularised sort borrows something from the structural proposition. (I think the distinction involved is that which Ockham draws between "pure propositions of logic", and mere illustrations of logical principles. The former contain only "terms of second intention", *i.e.*, technical descriptions of the "modes of signification" of other terms, while the latter contain "terms of first intention", *i.e.*, terms capable of referring to "real" objects, such as the moon. But while the peculiar terms of the "pure propositions of logic" are used in an ordinary way, *i.e.*, to refer to their own appropriate objects, the ordinary terms of logical illustrations are used in a peculiar way, "non-significatively", not to refer to their objects, but merely to typify their own modes of signification, so that the substitution for them of other terms of the same sort would leave the force of these propositions unaltered. See Moody, pp. 33-34).

Let us now take another syllogism of the same sort, but simpler:

> Any proposition is either true or false, but not both.
> That *matter exists* is a proposition.
> Therefore, that *matter exists* is either true or false,
> but not both.

Since neither of the two premisses is really necessary as a premiss, we may in a sense derive the conclusion from either taken alone. We derive it from the first by the immediate employment of the "applicative principle", and from the second, I think, in some such way as this: We may ask what we say of matter exists when we say that it is a proposition; and the answer is that we place it in a category of subjects determinable in respect of, among other things, something that we might call "factual reference", which has two and only two determinate forms, truth or the representation of fact, and falsehood or its misrepresentation. And this is what the major premiss says of *every* proposition.

That major premiss simply sums up, of course, those generalised formal propositions which we call the Law of Excluded Middle and the Law of Contradiction. In Johnson they are called respectively the Alternative and the Disjunctive Principles of Propositional Determination (I. xiv. 6). But I have now worded them in such a way that they may also be regarded as illustrations of what he calls the Alternative and the Disjunctive Principles of Adjectival Determination, which might be jointly rendered, " Any subject given to be characterised under a certain determinable, is characterised by one and not more than one of the determinates under that determinable ".

Johnson himself regards the propositional principles as the most general " laws of thought " of which the others are special applications. In this I think he is in a measure right ; but his particular way of moving from the propositional principles to the adjectival involves a doubtful step. Between the propositional principles and the final adjectival ones he has interposed a set of preparatory adjectival ones, in which the subject is neither " Any proposition " nor " Any subject determinable in a certain way ", but simply " Any subject ", and it is questionable whether these ones have, on his own principles, any meaning. This may be said, at least, of his intermediary version of the Law of Excluded Middle.

It has sometimes been argued against the Law of Excluded Middle that some propositions are neither true nor false but meaningless. I do not think there is anything to be said for this ; it seems better to say that what is meaningless is not a proposition. But for reasons suggested in an earlier section, it might plausibly be held that it is meaningless to say that what is meaningless is neither true nor false, and that in consequence the Law of Excluded Middle is itself meaningless in the form " Anything must be either p or not-p ", where p is any adjective, though in the form " Any proposition must be either true or false " it is true without reservation. Johnson " deduces " the former formulation from the latter thus : Of any proposition P it is true that P must be either true or false. Let P, now, be " taken to stand for a definite singular proposition ' s is p '," where p stands for " any adjective ". We may then say that s must be either p or not-p. We may say this, because it is only another way of saying that it is either true or false that s is p. We may generalise this result to " Anything must be either p or not-p ". Hence, among other things, a table must be either true or not-true. But is Johnson entitled to assume that, given a definite subject s, we may say that it is either true or false that this subject is p, where

p is any adjective? This step seems to me a doubtful one because the original law was that any *proposition* must be either true or false, and if we select an adjective which has no application in the category to which the given subject belongs, the resulting construct might not be a proposition. But given any adjective p which has application in the category to which the subject s belongs, the assertion that s is p will be a proposition, and will therefore be either true or false. And since the absence of p in a subject belonging to the category in which it is applicable is invariably due to the presence of some other character under the same determinable, we may pass from this to the adjectival principle that whatever is determinable in respect of, say, P, is either p or p' or p'', etc., where these are determinates under P. And since propositions with subjects in any category are themselves in the one category of "propositions", the propositional principle is illustrated by the adjectival one as formulated for any category. In this sense the propositional principle is, as Johnson contends, more general than the adjectival one. It says that, given propositions with subjects in any category, so long as their predicates may be affirmed or denied in that category and the resulting constructs therefore are propositions, these propositions are either true or false.

On the other hand, since the category of propositions is itself one among others, we may also regard the propositional principle as illustrating the adjectival, as I have earlier argued. Once again, there seems to be a hint of this point of view in William of Ockham. Ockham's logic is primarily a logic of terms, dealing with such properties of propositions as arise from the relations between their terms. "Signification", he insists, is prior to "predication" (Moody, pp. 68 ff.). But when he comes to deal with what Aristotle called "topical" reasoning, he deals with this as *reasoning in which propositions themselves are taken as terms* (pp. 283-284), and it is in this part of his work that we find his most striking anticipations of modern formal and symbolic logic.

The truth seems to be that Johnson's propositional and adjectival principles have, as Presbyterians say of Church and State, "co-ordinate jurisdiction with mutual subordination"; so that each of the two kinds of logic has a place in itself for the other.

NOTE.—The problem of fitting the relation between determinates and determinables into a purely "conjunctional" logic might be summarily described as the problem of justifying the inference from "This is red" to "This is coloured" on the

assumption that all formal inference consists in the passage from a conjunction to one of its conjuncts. And a possible solution to this problem which I may appear to have ignored would lie in the simple denial that " This is coloured " does formally follow from " This is red ", *i.e.*, from " This is red " alone. On this view, the apparent validity of this inference is to be accounted for by the fact that when we make it we always have in the back of our minds an additional premiss.

The only suppressed premiss that I can think of which would make this theory plausible, is " Red is a colour ". The inference is then reducible to the form

> Redness characterises this and is a colour;
> Therefore some adjective characterises this and is a colour

(where the premiss is a paraphrase of " This is red and red is a colour ", and the conclusion a paraphrase of " This is coloured "). The justification of this inference from a singular proposition to the corresponding particular is perhaps best made clear by obverting both the premiss and the conclusion, thus:

> Redness does not characterise this without being a colour;
> Therefore not every adjective which characterises this is not a colour.

What we now have is simply an indirect or contraposed employment of the Applicative Principle (using the term contraposition, as Dr. Keynes uses it, for the passage from " If p then q " to " If not q then not p ", as well as for the allied movement, from " Every P is Q " to " Every non-Q is non-P ", to which it is more commonly applied). Because we may pass from a universal proposition to the corresponding singular—from " every " to " this given "—we may also pass from the singular which contradicts the latter to the particular which contradicts the former—from " not this given " to " not every ". This indirect employment of the Applicative Principle has already been illustrated in the analysis of third-figure syllogisms—in the step from " The moon is not uneven even if it reflects the light, etc.," to " Not every body which reflects the light, etc., is uneven ".

Obversion, contraposition and the Applicative Principle do not, on the face of it, present any difficulties to a conjunctional logic. The validity of obversion and contraposition is demonstrably part of what is meant by saying that every proposition is either true or false and none is both, and the passage, in the employment

of the Applicative Principle, from a predication about " every " to the same predication about " any given ", seems to be simply a passage from an infinite conjunction of predications about " given " subjects to a particular one of these predications—though the sense in which redness, say, is " given " whenever we speak of " every adjective ", needs to be carefully defined. (We may say, broadly, that it is " given " in so far as the phrase " every adjective " presents to us the entire category or universe in which redness is an item).

" This is coloured ", then, formally follows from " This is red and red is a colour ", even when " formally " is given a strictly " conjunctional " interpretation; while it does not formally follow from " This is red " alone, in the same sense of " formally ". It follows in the same way as " A man is in this room " follows from " Plato is in this room and Plato is a man ". We might display this latter inference also as a " contraposed " employment of the Applicative Principle, thus:

Plato is in this room and is a man;
Therefore some individual is in this room and is a man,

or (obverting)

Plato is not in this room without being a man
Therefore not every individual that is in this room
is not a man.

The representation of the inference from " red " to " coloured " as a formal parallel to this one, is, of course, in accordance with our earlier treatment of determinates under a determinable as constituting a class of adjectives or qualities. In fact, our final comment on this representation of the inference must be simply that it takes us just as far as, and no further than, this treatment of the determinable as a class-name.

And how far, exactly is this? If colours do form a class, it is a second-order one—not, indeed, a class of classes, but a class of defining characteristics of classes. And the defining characteristic of this class itself, the characteristic of redness that places it in the class of colours, is just that it cannot be significantly predicated of any subjects but " coloured " ones. In other words, to say that red is a colour is just a way of saying that " This is coloured " follows from " This is red " alone, in the sense that the truth of the former is presupposed in the significance of the latter. " Red is a colour " is therefore a sort of super-major in the logic of determinables; and the existence of a logic with such super-majors is already assumed in the above-outlined

attempt to justify the inference of "coloured" from "red" by the methods of the other logic.

An alternative suggestion which a "pure conjunctionalist" might make is that "This is red" implies "This is coloured" because it means the same as "This is coloured and not orange and not yellow and not green etc.," (going through all the colours but red). But while such an expansion of "This is red" is, at its own level, legitimate, its legitimation cannot come from a logic which sets out merely from the facts that every proposition is either true or false, that none is both, and that a group of propositions are not jointly true if any one of them is false. Such an expansion, in other words, is not merely an elaboration of what it means for "This is red" to be a proposition. We can, indeed, even in a logic thus limited, expand "This is red" to "This is not not-red" (*i.e.*, to "It is false that it is false that this is red"), and this in turn may be expanded to such forms as "This is not both not-red and square, nor is it both not-red and not-square". Moreover, "This is not not-red" may be regarded as at bottom just a briefer version of "This coloured thing is neither orange nor yellow nor green, etc.," ; but this last equivalence can only be seen with the aid of the "logic of determinables". (As we might also say: it is not a purely formal equivalence, in the sense in which "formal" has been used here).

IV.—ECONOMIC BEHAVIOUR AND WELFARE.

I. M. D. Little.

I

Philosophers interest themselves in the assumptions and methods of various branches of the natural sciences. Economics appears to be an exception, in spite of the fact that economists, like physicists, often talk philosophy. Nevertheless, I propose in this article to discuss, first, the theory of the individual's economic behaviour, and, secondly, the theory of economic welfare. These are, I think, the topics which might make the philosopher complain a little suspiciously that he cannot understand what Economics is about. The theory of economic behaviour has been thought to be an essential part of two important branches of Economics, the theory of Value and the theory of Economic Welfare. The so-called theory of value is the theory of the determination of prices. The theory of economic welfare is about the happiness of individuals and communities. I shall be concerned with, first, the correct interpretation of the theory of economic behaviour, secondly with the relation of this theory to that of value (price) and that of welfare, and lastly with the theory of welfare on its own account.

Economics has been held to be the study of the devotion of scarce resources to given ends. Therefore, that part of a man's behaviour which is called 'economic' must be assumed to be directed towards the attainment of some end. This end has been commonly held to be the maximisation of his satisfaction. This is a statement of the orthodox view. If a man's behaviour was quite wild and irrational, how could one say anything about it? It is in this way that the concept of a maximising individual, the famous 'Economic Man', has arisen. This maximising assumption is usually held to be the fundamental assumption of Economics.

The question as to what 'the maximisation of satisfaction' is supposed to mean must be discussed. Until recently, it was assumed that satisfactions were extensive magnitudes, and so they could be added. Thus 'the maximisation of satisfaction' was equivalent to 'the maximisation of the *sum total* of satisfactions'. But recently it was realised that it was not necessary

to regard satisfactions as additive ; all previous conclusions about economic behaviour could still be deduced if they were merely regarded as orderable. So it was assumed that the individual was trying to maximise his satisfaction only in the sense that he was striving to attain *the highest possible level* of satisfaction.

The next question we must answer is, ' Why is it necessary to assume that the individual maximises satisfaction in any sense ? ' It isn't. But the reason why it has been considered to be necessary is that it was thought that otherwise a definite solution to the problem of what the individual would do when faced with a particular economic situation could not be given. In order to explain this, I shall paint a metaphorical picture of the pure theory of individual economic behaviour. The interpretations of this picture which are not here disputed are given in brackets. The picture is as follows :—the individual (individual) is seen to be taking a walk along a mountain path (the co-ordinates of each point on the path are determined by the economic possibilities, *i.e.* by the man's resources and the prices of the available goods. They represent various combinations of goods which he could just buy). The problem is to determine the point on the path at which he will stop to rest (what collection of goods he will buy). The ' fundamental ' assumption of Economics is that the individual won't fall off the path, and that he will stop to rest *at the highest point the path reaches.*

In the above picture, the interpretation which is under dispute is that of the height dimension. On the orthodox view, the mountain is called a preference field ; the contours are called indifference curves, and they mark different levels of satisfaction. The metaphor breaks down in one respect, because the contours are not marked in feet (units of satisfaction), but are merely assigned ordinal numbers. Thus it would be impossible to postulate that the individual rests, for instance, half-way up, for there is no way of determining the half-way height. There are two possibilities : either he never starts at all (and the interpretation of this would be absurd) or he stops at the highest point. Hence the reason why a maximising postulate is found necessary in the pure theory is obvious. But, as I indicated, the interpretation is also in terms of maximising. Indeed, as far as I know, the pure theory and its interpretation have never been separated in economic literature. I wish to show that an interpretation can be given which avoids not only all mention of satisfaction but also all mention of maximising.

First, however, we must discuss what is wrong with the notion of maximising satisfaction. Now economists certainly hold that

their theory only applies in so far as men are rational; by 'rational' in this context they can, on their own principles, only mean satisfaction-maximising. Therefore, if we want to know how far the theory applies, and when to apply it, we must have some test of when an individual is maximising his satisfaction. Now there are, no doubt, tests for when an individual is doing this, but they are tests which would lead to the discovery of selfish hedonists. Economists have often been accused of theorising only about such unpleasant people. Moreover, they have only themselves to blame for this absurdity, for the truth of the matter is that there are no tests for satisfaction-maximising which an economist would accept. One might pick on a person who was obviously making himself as miserable as possible, deliberately. But the economists, *if certain conditions were fulfilled*, would have to refuse to accept that he was doing anything other than the precise opposite. What these conditions are will emerge.

The interpretation in terms of satisfaction-maximising is clearly wrong. An interpretation in terms of maximising anything else, girth, family, or misery, would be no better. If satisfaction (or happiness) has to go, then maximising must go as well. A clue to the correct interpretation may be discovered by scrutinising the method by which the economist proposes to draw contour lines on the individual's map. First, a particular collection of goods is described. Then by questioning the individual, every collection of goods, such that he declares himself *indifferent* between each of these and the original collection, is discovered. On a graph, the line which connects these collections is the contour line, or indifference-curve, on which all collections are iso-satisfactory. Then another collection, declared to be preferable to the original, is taken, and the same procedure is applied. Thus, so long as preference is transitive, and so long as 'A preferred to B' and 'B indifferent to C' together entail 'A preferred to C', then the whole map can be covered with contours.

Now the method described above unfortunately demands a subjective interpretation of the contours, because indifferent behaviour has not been defined; nor could it easily be defined.

It is not implied that there is anything fundamentally subjective about indifference. It is simply that there is no obvious definition of indifferent behaviour which is convenient for economic analysis. As a result, for reasons of symmetry, 'I prefer' must also be interpreted subjectively, and be taken as equivalent to 'I like better'. But 'I prefer A to B' can also be taken to mean 'I would choose A rather than B if I had the choice'. Therefore, a slight amendment of the above theoretical procedure can, by

banishing ' indifference ', result in a purely behaviouristic interpretation of the abstract system. The individual could simply be offered the choice (actual or hypothetical) between all possible pairs of collections of goods. The contour lines then become boundary lines between areas representing collections chosen rather than a given collection, and rather than which a given collection is chosen. The word ' indifference ' does not occur.

The question of what has happened to our maximising postulate must now be answered. If the choices made were hypothetical, then ' the highest point the mountain path reaches ' comes to mean ' that collection of goods which is, in the circumstances, the individual's first choice, as determined by his announced order of choice '. The ' maximising ' assumption is, therefore, that the individual behave in the announced manner. If the choices were actual, then the ' maximising ' assumption is that the individual continue to behave in the same manner. Neither assumption has anything to do with maximising. The contour lines can also, ideally, be drawn by observing the individual's actual market behaviour. There is no need to put him in a laboratory. Given this method, the highest point on the path is simply the same collection of goods which the individual has already chosen in exactly the same circumstances on some previous occasion. All that we require, therefore, to apply the theory is a *consistent* individual. Consistency here means two things: First, the individual if he ever chooses A rather than B must always do so. Secondly, his choices must be transitive. This is the ' New Economic Man '. He is not a very likely sort of fellow. Let us consider him briefly. The collections of goods referred to above must be interpreted as the collections of things bought in a given period of time. Now if prices and his income did not alter, our economic man would buy exactly the same collection of goods in every period. To take a short period is ridiculous. No one would buy the same collection of goods week after week. If we take the period as a year, it is a little more plausible. Even so, his tastes must not change, and he may never buy a novelty. He still rides about in a cab.

Whether this new economic man is plausible or not, is a matter of no importance to the positive science of Economics. It would not be a practical matter to chart a man's behaviour, even if he were consistent. As far as I know, not even one contour line for one individual has ever been successfully drawn. The result is that the theory of economic behaviour consists only of a skeleton, which is a statement of how an individual must behave if it is to be theoretically possible to predict his purchases from a knowledge

of the amount of money he has, and the prices of the available goods (together with a few rather trivial conclusions which follow from these conditions alone). The 'fundamental' assumption of Economics is, in fact, analogous to the statement that one cannot draw a map of shifting sands. Since the sands do shift, Economics must be a very shaky science if this really is a fundamental assumption. But, of course, it is not. Positive economists are not interested in individual consumers, but in demand. Demand is determined by many individuals, and, fortunately, this mass of individuals behaves more consistently than the atoms of which it is composed. The basis of the theory of demand is the demand-curve, which states a functional relationship between the price and the quantity of a commodity demanded by the aggregate of consumers.

It has hitherto seemed that Economics and Psychology could find a meeting-place in the study of individual behaviour. Economists have proposed a general psychological law, the so-called law of diminishing marginal utility—the more you have of a thing the less you would like a little bit more. But all the behaviour restrictions implied by this proposition can be deduced from consistent behaviour together with the postulate that a person chooses a larger collection of things rather than a smaller. One collection of things is said to be larger than another if there is more of one kind and no less of any kind. (In the pure theory it is a tautology that the individual chooses a larger collection of *goods* rather than a smaller.) Thus the more primitive behaviouristic law, that a person chooses the larger rather than the smaller collection, can take the place of the usual psychological, and almost certainly less generally valid, law of diminishing marginal utility. It seems most improbable that any psychological law, discovered by introspection or clinical research, will ever be able to tell the economist anything about demand which he cannot more easily discover from direct statistical inquiry, or ordinary observation.

There is, however, one sphere in which the study of individual behaviour may seem of importance to the economist, and that is in the Theory of Economic Welfare. For here ordinary observation, introspection, and psychology do provide our data. This brings us to the second part of the article.

II

The Theory of Economic Welfare.

Economists are primarily interested in the welfare of the community, and not the welfare of particular individuals. But the welfare of the community is regarded as a logical construction from that of individuals. Therefore, some assumptions about individuals are essential to the welfare theorist, although not as we have seen to the demand theorist.

Professor Pigou's book *The Economics of Welfare* (First Edition, 1920) is the last and greatest discussion of the theory of economic welfare in the utilitarian tradition. The economic welfare of an individual was held to be 'the part of welfare which could be brought into line with the measuring rod of money'.[1] Thus welfare was regarded as the total amount of satisfaction the individual had, and economic welfare was that part of this total which was, roughly speaking, the net effect of the consumption of goods and services, and the work done. The economic welfare of the community was the grand total summed over all its members. In this theory it had to be assumed that each individual was a maximising unit, otherwise any measure to increase the total welfare might be baulked by an obstinate refusal of individuals each to maximise his own sub-total.

If it was assumed that an extra pound would give the same satisfaction to everyone, a number of 'optimum' conditions for the organisation of production and distribution could be deduced. There is nothing trivial about these conditions. They would be exemplified in a purely competitive (though not in a *laisser-faire*) society. What exactly is meant by a purely competitive society would be too long a story. It suffices to say that any attempt to bring them even very approximately into realisation in any actual society would require far-reaching changes, and much central control.

Thus the theory of welfare, whatever its ethical implications, was supposed to be a science which could tell one which changes would, and which would not, increase the size of the pool of satisfactions, named 'The Welfare of Society'. This theory has now been renounced by all, or almost all, economists who write about welfare. We have seen that the idea of satisfactions as extensive magnitudes was given up. This meant that satisfactions could no longer be added, even for one individual, let alone for lots of individuals. But the theory was also renounced because the idea that the satisfactions of different people were

[1] *The Economics of Welfare*, Fourth Edition, p. 12.

comparable was severely attacked. It is, of course, true that satisfactions are not measurable, and also true that there are no generally agreed tests for deciding whether a pound means more to A or to B, but some economists, beset by metaphysical doubt, deny that different persons' satisfactions are, even in principle, comparable. Apart from these reasons, it may be added that the Pigovian theory of economic welfare required that satisfactions could be allotted to particular causes. This satisfaction was the effect of an economic cause, that the effect of a political cause, and so on. The resultant picture of a lot of separate satisfactions, each with its own label, is not very convincing.

The actual criterion used for an increase in individual welfare did not alter as a result of these criticisms. In the metaphor used above, the criterion is that the individual be higher up the mountain. But the interpretation altered. This no longer meant that he had more satisfaction ; instead it denoted a higher level of satisfaction. But the proper interpretation of the criterion (the practical test is ' Could the individual have bought the goods of the previous period in this the succeeding period ? ' If he could have and doesn't, then he is ' better off '. The fulfilment of this practical test entails a higher position ' on the mountain ') is that he is in a *chosen* position. This criterion will be called the choice criterion.

What could be said about the welfare of the community in the light of the criticisms made of the Pigovian theory ? One line of possible development may be sketched. The fulfilment of the choice criterion could be taken to imply that the individual had more satisfaction. It could then be said that, in describing changes in the size of community welfare, one was describing changes in the size, not of a homogeneous, but of a heterogeneous collection of satisfactions. Such a collection can only be said to be larger than before if at least one item is greater, and no item is less. Therefore, the criterion for an increase in community welfare which should have emerged if economists thought that the Welfare of Society is a name for a collection of individual satisfactions, or happinesses, is that at least one person must be ' benefited ' and no one be ' harmed ' (' harm ' and ' benefit ' being determined by the choice criterion).

The new welfare economists (as they will be called) have not taken the above line of development. For the individual, the choice criterion is used.[1] But for the community the following

[1] But, strictly, the new welfare economists could not call it a choice criterion, for the theory of individual economic behaviour they relied on could only (as we have seen above) be interpreted subjectively.

criterion was proposed, and seems to be widely accepted. The economic welfare of the community can be said to have increased if (*a*) someone is benefited and no one harmed, or if (*b*) the gainers *could* compensate the losers, at least one person remaining a beneficiary. The compensation suggested is hypothetical, so that welfare can be said to have increased although some people (or even all but one person) are harmed. Thus the new welfare economists do not describe changes in the size of a collection of satisfactions, for they permit losses and are not utilitarians. Their procedure, in fact, is to take a common phrase and give instructions for its technical use. They take little notice of its ordinary meaning.

We may now discuss these two criteria which are fundamental to the modern theory of welfare. Let us, first of all, deal with the choice criterion, which says that an individual's economic welfare is greater this year so long as he could have bought last year's goods if he had wanted to. Now what does the statement ' John's economic welfare has increased ' mean in ordinary language ? It seems that it is reasonably nearly equivalent to ' the consumption of goods and services and the work done in the community has changed in a manner favourable to John's happiness '. But the fulfilment of the choice criterion only weakly implies the above statement. This shaky implication would be greatly strengthened if certain assumptions could be validly made ; if it could be assumed that John's happiness was independent of the economic circumstances of others, and if it could be assumed that his tastes had not changed, and that he always chose wisely. These and other qualifications have, however, been generally recognised by economists. But it, nevertheless, remains true that they tend to be obscured by the laying down of a precise sufficient criterion for an increase in the individual's economic welfare. What has not been noticed at all is that ' John's economic welfare has increased ' is a statement with a slight ethical flavour about it. There is, I think, the same flavour about my translation, to wit ' the consumption of goods and services and the work done in the community has changed in a manner favourable to John's happiness '. This is the case because most people think it is, other things being equal, a good thing to increase a person's happiness. Thus if the choice criterion misleads as to matters of fact about the causation of happiness, it also tends to induce people to give their approval to changes which otherwise they might have rejected.

We can conclude that the choice criterion is not acceptable as a sufficient criterion. No one would want to say that a person

must be better off economically because he is in a chosen position. One cannot allow ' economic welfare ' to be partially defined in a manner which diverges from ordinary usage, because it is an emotive expression. Nevertheless, the choice test may be acceptable as a fairly good guide to changes in welfare over a short period of time. If we only put it forward as a suggested guide, and not, more pretentiously, as a criterion (or even a definition), there should be little to complain of.

We have seen that the statement ' John's economic welfare has increased ' has a fairly precise everyday descriptive meaning. But this is not the case with ' the economic welfare of the community has increased '. This statement could be given a similar translation to the former, *e.g.*, ' economic circumstances have changed in a manner favourable to the happiness of the community ' ; but this does not help, because ' the happiness of the community ' is not a phrase with any precise generally accepted meaning. The factual reference of such a statement is, in ordinary language, very vague, and varies from person to person. It is in such vague phrases that we are most likely to find strong emotive content. If a word has a precise descriptive meaning, it is unlikely to be very influential. The emotive attitude is linked to what the word describes, and any deviation from the correct usage of the word is easily noticed. But, where words have no common descriptive meaning, or only a very vague one, they are particularly liable to be persuasive, irrespective of what they are being used to describe. In passing from the welfare of the individual to that of the community, the reference to fact has become exceedingly vague, and, at the same time, there has been a very considerable expansion of emotive content. The statement ' this change would increase the economic welfare of the community ' is undoubtedly strongly recommendatory. It must also be remarked that, after the first page or so, the word ' economic ' is almost always left out by economists. Of course, there is no *misunderstanding* as a result, but, nevertheless, the effect on the reader is altered. Typical phrases in use are ' socially advantageous ', ' increase of social welfare ', ' social benefit '. The emotive force is greatly increased by the addition of the word ' social ', as well as by the subtraction of the word ' economic '. If I say ' economic welfare would be increased ', I leave an open door. Someone can reply ' Ah ! but political welfare and cultural welfare would be greatly diminished '. If I leave out the word ' economic ', I am placing a screen in front of the door, and, as a result, my statement becomes much stronger.

We may conclude that the deductions which welfare economists

make are quasi-ethical. They are certainly much more nearly akin to pure ethical statements than they are akin to statements of fact. Now there is nothing wrong with pointing out what follows from accepted value premises. The theory of economic welfare could be regarded in this light if the premises, *i.e.* the two criteria given above, were acceptable. Acceptable to whom? Perhaps the best we can do is to make a guess as to whether a majority would accept them to-day.

The criterion for an increase in an individual's welfare we have already discussed. We decided that it could be called a guide to welfare, but not a sufficient criterion. We may repeat the criterion for an increase in general welfare. The welfare of the community has increased if the beneficiaries could compensate the losers, at least one remaining a beneficiary. In this criterion, the losses and gains of individuals are determined by the choice criterion. The weakness of the latter is thus included in the general criterion. But, for convenience, we will suppose that the choice criterion is acceptable, and, therefore, speak without qualification of 'harm' and 'benefit' with reference to individuals.

Is then the criterion for an increase in the welfare of the community acceptable? The compensation for losers is only hypothetical. Thus a change which made the poor much poorer, and the rich so much richer that they could compensate them, would be said to cause an increase in the economic welfare of society. Thus the criterion takes no account of the distribution of wealth; its failure to do so surely means that it would scarcely be acceptable to anyone. No one would say that an economic change which would be thought evil would increase the economic welfare of society. This unacceptable premise is used for the deduction of the same 'optimum' conditions of production and distribution as could be deduced from the utilitarian theory on the assumption that a pound meant the same to everyone. But this latter assumption was admittedly invalid, and so the welfare conclusions had to be qualified by a reference to the distribution of wealth. The new welfare theory escapes this reference, congratulating itself on avoiding vague and normative propositions about the distribution of wealth; and claims, therefore, to have cut itself off from unscientific ethical questions, and to be a purely objective, scientific study. Such are the miracles which can be accomplished by a good *objective* persuasive definition!

The criterion for an increase in general welfare under discussion has been enthusiastically accepted by most welfare writers, because it permits the valid unqualified deduction of the 'optimum' conditions. But there has been very little attempt to justify it.

Scarcely anyone seems to have realised that some justification is required. It has, however, been suggested that if every change which was an improvement by the criterion was made, then, although each such improvement would harm some people, nevertheless, after the lapse of a sufficient length of time, it would be probable that almost everyone would be better off (*i.e.* on a higher level of satisfaction, or, on my interpretation, in a chosen position). Now this is the kind of reason which might make the conclusions of welfare economics acceptable, *i.e.* acceptable as a result of following from acceptable premises, and not merely as a result of being couched in emotive terms. But the justification appears full of holes when one looks at it closely. If one has to appeal to a fairly long period of time, the acceptability of the choice criterion is greatly diminished, because over a long period of time an individual's tastes change (as he gets older). So long as I could buy the goods I bought ten years ago I am now said to be better off (in a chosen position) than I was ten years ago. But this is not plausible if I have ceased to like that kind of thing. Furthermore, over a long period of time, the economic circumstances of others may change considerably, and it is only an approximation to suppose that my satisfaction is independent of their circumstances. Again, over a long period, many new kinds of goods are introduced to the market, and some drop out. If at two dates very different kinds of goods are available, one cannot make a plausible welfare comparison for the individual simply by studying the things he buys at both dates, and their prices. If we were dealing with an unchanging group of individuals it might be true that certain recommended changes would make almost everyone better off (in the everyday sense) in the end; but it would certainly not be true *because* some precise criterion was fulfilled for almost every individual. To compare a man's economic welfare with that of twenty years ago is more like comparing my economic welfare with yours than it is like comparing a man's circumstances over a short period of time, even although in the former case I have your opinion as additional evidence. But this kind of inter-personal comparison is ruled out of court by the new welfare economists. It is not scientific or objective. Again, over a long period of time, the implicit assumption of the justification under consideration, that one is dealing with an unchanging group, becomes absurd. Half the people who were going to become better off in the end would be dead.

It must be emphasised that the objections listed above do not make the justification *absurd*. They only make it absurd when

coupled with the claim that welfare economics is a precise objective science about what is ordinarily called welfare. Whether the justification is, apart from this claim, regarded as a good one is a matter for individual judgment.

The ' optimum ' conditions can also be derived from the much more acceptable criterion which lays it down that the welfare of society has increased only if someone is benefited, and *no one* harmed. It would then be required that everyone who loses as a result of a certain change must actually be compensated. There is, however, a very good reason why this criterion has not been made the basis of the new welfare economics. It could never be fulfilled. Actually to pay compensation, and to pay just the right amount (to pay more might exhaust the gains before all the losses were compensated) would require complete knowledge of everyone's behaviour map ; but nothing is known about anyone's. Any economic change has very widespread effects because all prices are, to some extent, interrelated. Apart from a few large losers or gainers, there would normally be many thousands of people who were very slightly affected. It would never be a practical possibility to make sure that no one lost. If one took the commonsense attitude of neglecting all very small individual gains and losses (to suggest even this is making a value judgment, albeit one that is likely to be accepted by most people) the theory would collapse, because in many cases a small number of fairly large losses would be offset only by a very large number of very small gains. This would indeed be the typical case. We must conclude that this criterion, the most promising from the point of view of general acceptability, although it suffices for the formal deduction of the ' optimum ' conditions, is a broken reed, because it makes the theory quite inapplicable. It may seem curious that the hypothetical criterion is a sufficient basis, when there are such difficulties about actual compensation. It is sufficient because the shapes of the contours of individual behaviour are irrelevant to knowing that compensation could be paid if they were known, and because it is tacitly assumed that it would not cost anything to find out these details, or actually to pay each individual the amount of compensation thus determined.

We may now consider a way of looking at the question of compensation which is popular with economists and which leans towards more practical issues.

It has been argued that the payment of compensation is a device for keeping the distribution of income constant, and thus (it is said) abstracting from the question which arouses the strongest ethical attitudes, while changes are made. This argument

has even been raised in support of the accepted criterion, that of requiring only hypothetical compensation. This would be true only if abstracting from a question meant ignoring it. But we have seen that it would be impossible to compensate all losers. Does this mean that this idea of abstracting from questions of the distribution of income is absurd? To answer this question we must first inquire into what is meant by the distribution of income.

The distribution of money income means something quite precise. But an interest in the distribution of money income is derived only from the belief that money incomes are correlated with *real* incomes, or *real* wealth (or welfare). This is easily demonstrated. Consider an inequilitarian society in which there are rich people who buy 'luxuries' and poor people who buy only 'necessities'. Let the price of 'necessities' rise, and that of 'luxuries' fall, and let there be no change in money incomes. One would now want to say that the distribution of real income had changed in favour of the rich. But real income comparisons between different people are not precise; nor are there any agreed tests for when one should say that A is really wealthier than B. It is maintained by one economist that all such judgments of comparative real income, and comparative happiness, are merely value judgments. That is going much too far. We do have tests for deciding whether A is really wealthier than B. But in some such cases people would disagree, and in many people would not want to come to any definite conclusion. However, the question whether A or B is really wealthier does not cease to be a question of fact, because it does not always have an answer, nor because it sometimes has two answers. Judgments about real income, or real wealth, distribution are essentially imprecise; and it is real income, not money income, judgments which are relevant to questions of welfare.

We may now return to the question whether the idea of 'abstracting from' income distribution by paying compensation is absurd. The answer is that it is a perfectly sensible idea. One can try to be pedantic and argue that the distribution of real income cannot be kept intact, because someone is to benefit while others are only just compensated, and because there are bound to be small uncompensated losers. But this is a quibble only valid against those who claim that the theory of welfare is precise and scientific. Whether the distribution of real income is kept intact is a matter of individual judgment. So long as large losers were compensated, I might well be prepared to say that the distribution of income was unchanged, *and* that some had benefited and others lost. I would not be contradicting myself,

because the judgment that the distribution of real income is unchanged is insufficiently precise to support a contradiction.

Keeping the distribution of income constant may prevent much opposition to a change. But sufficient opposition may be overcome by seeing that the distribution of real income changes in an approved manner. We could then propose a new guide to welfare. The welfare of society will probably increase if the gainers could compensate the losers, and if the distribution of income does not change enough to arouse too much opposition, or if the distribution of income changes in a manner which most people approve of.

A test such as the above would not admit any change generally held to be bad. It would, however, be very difficult to say why the changes which were let through by such a criterion were good. Furthermore, it must be recognised that this test would be condemned as quite useless by the new welfare economists. It would not enable them to say precisely and objectively which changes would, and which would not, increase the welfare of society. But is that, after all, such a disadvantage ?

Economists are coming to have a larger say in practical matters. Whenever they argue for or against some policy, speaking purely as economists, their arguments are based on the theoretical foundations discussed above. These foundations are very shaky. It does not follow that there is nothing in what they say. More or less the same recommendations have been made from quite different conceptual bases for a long time, and it would be surprising if they were utterly worthless. The above discussion has been intended at once to prick the bubble, but, at the same time, to leave the impression that there is something in what they say. Exactly what that something is it is impossible to describe. When one is dealing with the happiness of the community, as soon as one aims at any precise statement, one must either get lost in a maze of contradictions, or else, in acquiring precision, one must lose almost all touch with the commonsense of the expressions one uses. Where these common expressions are charged with emotion, to lose touch with their descriptive reference, vague though it may be, is most dangerous and misleading. Welfare economists have committed this fault, and consequently misled themselves and others into accepting precise conclusions deduced from unacceptable or inapplicable premises.

The above situation seems to have arisen for two reasons. First, there has been too great a preoccupation with abstract analysis. Much has been written about the formal deductions which can be made, and much about particular applications of

the system. The Utopia which would result from a wholesale application of the theory has been described. But practically nothing has been said about whether the application of the system to any real community is justified, *i.e.* whether the system is really about welfare in the ordinary sense at all. The reason for this is largely that economists have failed to recognise their system for what it is, an attempt to build a practical scientific *ethic*. It should however be noted that economists, when discussing the best thing to do *in some particular case*, usually do bring in ethical considerations which lie outside the scope of the theory of economic welfare as described, and do not then claim precision. Such discussions are certainly valuable, and in some cases it can hardly be doubted that economists are better placed than others to judge what would be the best thing to do. What is here maintained is that the existence of a system of welfare tends to result in considerable emphasis being placed on the so-called optimum conditions, when the basis of that system is too shaky for more than a little weight to be justifiably attached to them.

The question may be asked, ' Could not the theory of economic welfare be called something else—and use only colourless language ? ' No doubt something could be done in this direction, but it would not be easy. If extreme tediousness of expression is to be avoided, certain portmanteau phrases must be used, and, given the history of the subject, it would be difficult to prevent the colour running back. Any such suggestion also ignores the fact that it is welfare people are interested in. They want to be told what they ought to do, especially when they do not realise that they *are* being told, and especially when the telling is done by ' scientists ' who tell them that they *know* the answers, because they have worked them all out. Of course, it might be best to pander to this weakness, for economists are surely not the worst witch-doctors. But, if it is not thought right to do so, then economists ought to make it clear that they have no theory of welfare. They may truly add that they have much to say that is relevant to welfare ; but they should not be tempted to begin, ' Speaking as a scientific economist, the best thing to do, from the point of view of economic welfare, is . . . '. That does not make sense.

V.—DISCUSSIONS

THE NATURE OF FORMAL ANALYSIS

SUPPOSE we have a verbal expression A. Then to say that A is a verbal expression is to say that there is some property or proposition which A expresses. And to say this is to say that A stands in a certain relation, namely that of expressing, to something else. The property of being a verbal expression is therefore a relational property. Let us represent the relation of expressing by the letter E, and let us take p as a variable whose values are properties or propositions. Then, to say that A is a verbal expression we can write $(\mathrm{E}p)A\ E\ p$; and to say of any specified property or proposition P that it is expressed by A is to make a statement of the form $A\ E\ P$.

Now a *usage* has two components, a verbal component and a conceptual component. The verbal component of a usage expresses the conceptual component; and the usage is the complex which comprises these two components related by the relation of expressing. That is to say, a usage is a relationship which is an instance of the relation of expressing. Let us consider an example. A grammarian might hold that the non-standard usages *Where are you going to?* and *where are you at?* were better than the standard ones, *Where are you going?* and *Where are you?* And he might hold this on the ground that, in a context involving also *Where do you come from?* we can put a univocal interpretation upon the word 'where' when it occurs as a component of these non-standard usages, but not when it occurs as a component of the corresponding standard ones. He would then be analysing certain usages for grammatical purposes. And note that the usages compared have the same conceptual components, but not the same grammatical components.

Now, just as we say that two verbal expressions are synonymous if and only if they express the same idea, so we may say that two usages are synonymous if and only if they have the same conceptual component. Thus the usage *where* and the usage *to what place* are synonymous when they occur as partial usages in *Where are you going?* and *To what place are you going?* But there are partial usages which occur in the latter and not in the former. Thus the partial usage of which the word 'place' is the verbal component, and which is also a partial usage in *The place you just mentioned is near here*, does not occur in *Where are you going?* despite the fact that the conceptual component of that usage, which is the idea of a place, does occur.

It sometimes happens that a partial usage is designated or referred to in another partial usage. Thus, in *Smith was so called because that was the name of his father* we have a curious combination of references. The first word in the sentence denotes a man, and does so by being

the verbal component of a usage whose conceptual component characterises uniquely the person in question. The phrase ' so called ' applies to the use of this word, which has just been exemplified, and therefore denotes a usage of which the word ' Smith ' is the verbal component. And the word ' that ' denotes the word ' Smith ' itself, and therefore denotes a word which is the verbal component of the usage referred to by the phrase ' so called '. When a word has just been used, we can of course refer demonstratively to that word, and if the word has been understood, we can refer demonstratively to the usage of which that word is the verbal component, even in one and the same sentence.

These remarks are designed to make clear the several concepts with which we are concerned ; and we may now turn to the notion of formal analysis. We may consider first an example which has been discussed by Professor Moore and others and which embodies in a simple way the characteristic features of a formal analysis.[1] This example is *To be a brother* is *to be a male sibling*, where the first italicised part of the sentence corresponds in some way to the analysandum and the second italicised part corresponds in that same way to the analysans. When we ask in what sense this statement involves an analysis, we come immediately upon the so-called paradox of analysis, namely, that if the analysandum and the analysans have the same meaning, the analysis is trivial, but that if they do not have the same meaning, the analysis is incorrect. For it is clear that to know that a person is a brother it is necessary and sufficient to know that the person is a male and is also not an only child ; and that is precisely what knowing that a person is a male sibling requires.

I wish to hold that when we give this analysis we are analysing neither a verbal expression nor a concept but a usage. And I wish to hold that we can express this analysis by saying that the usage *being a brother* is synonymous with the usage *being a male sibling*, where the first italicised part of the sentence is the verbal component of the analysandum and the second italicised part is the verbal component of the analysans.

Now, only certain features of symbols are symbolically significant ; and, unless a symbol is ambiguous, not more than one concept will be expressed or symbolised by the fact that a symbol has a certain feature. Thus there is a feature of the expression ' being a brother ' by which this symbol expresses the property of being a brother ; but there is no feature of the symbol by which it expresses the property of being a sibling, or by which it expresses the property of being a male, or by which it expresses the idea of logical conjunction. Yet all these concepts are components of the property of being a brother. On the other hand, the expression ' being a male sibling ', which likewise expresses the property of being a brother, has a component which expresses the idea of being a male as well as another component

[1] See " The Philosophy of G. E. Moore " in *The Library of Living Philosophers*, vol. 4, pp. 660-667.

which expresses the idea of being a sibling ; and it has also an expression for the conjunction of these two ideas, which consists in the juxtaposition of the corresponding symbols.[1]

If in a verbal expression which is the verbal component of a given usage there is a subordinate symbolically relevant component, we say that it is the verbal component of a partial usage with respect to the total usage in question. And in a formal analysis, in which the analysandum and the analysans will be synonymous usages, to every partial usage of the analysandum there will correspond a synonymous partial usage in the analysans, but there will exist at least one partial usage of the analysans to which no partial usage of the analysandum corresponds. When these conditions are fulfilled, the analysans will analyse the analysandum, and a statement to the effect that these two usages are synonymous will be a statement of the analysis.

We have in formal definitions, where undefined characters are introduced, many examples which, when appropriately reinterpreted, provide examples of formal analyses. Suppose we give a formal definition which introduces one or more single characters in place of corresponding complexes of characters. These new characters, or such of them as are new, need not be given any meaning at all, but can be used merely as items to be transformed into meaningful expressions at will. After frequent uses, however, we shall come to put upon the new characters of the definiendum those interpretations which we put upon the corresponding complexes of characters in the definiens. The two expressions will then have become synonymous ; and reading the definition as *The usage X is synonymous with the usage Y*, where the verbal expressions employed are the verbal components of the analysandum and the analysans respectively, will be an expression of an analysis. It will be true, of course, that the analysans was known beforehand, and that our knowledge was therefore not advanced by the analysis. Nevertheless, to say, for example, *The usage* $P \supset Q$ *is synonymous with the usage* $\sim P \vee Q$ will be to express an analysis, provided the two symbolic expressions are independently interpreted.

It may be remarked that when we feel it difficult, as we sometimes do, to determine whether a word is being used autonymously to refer to itself or is being used to refer to something else, the fact is that neither is the case, but that the word has an occurrence as a component of a usage. And sometimes a word just occurs without being used at all, either as a designation of itself or as a designation of something else or as a component of a usage, as in *How do you spell Ann Arbor ?* For I can spell ' glub ', which has no use and is without sense. And the strangeness of a question like *How do you spell Ann Arbor and is it a large city ?* comes from the fact that we are required to give a meaning to a word which, when it was presented, had none, and not from the fact that we are required to give a double sense to a word.

[1] *Cf.* Moore, *loc. cit.*, p. 666.

We may consider a further example, which was actually given as an analysis, and which is clearly a formal analysis. This is the example which occurs in Russell's well-known doctrine about the use of ' the ' in the singular, and which concerns statements of the form *The x which has F is G*. The use of the word ' the ', according to Russell, expresses unique characterisation ; and the condition that a property F shall characterise uniquely anything it might happen to characterise is

$$(x, y)\ (Fx\ \&\ Fy \supset x = y).$$

Of course this does not say that F characterises anything ; but if we add $(\mathrm{E}x)Fx$, we have a statement to the effect that F has a unique application. And just as ' an x which has F ' is synonymous with ' Fx ', so ' the x which has F ' is synonymous with

$$'\ Fx\ \&\ (y, x)\ (Fy\ \&\ Fz \supset y = z)\ '.$$

In English, however, to put the verb ' to be ' in the indicative mood between a subject and a predicate term is to express the applicability of the conjunction of the concepts expressed by the terms. Hence, in ' (The x which has F) is G ', or ' The F is G ', the part in parentheses expresses the conjunction of the property of being F with the proposition that F is not multiply applicable, and the verb expresses the joint applicability of the subject and predicate concepts. What Russell's analysis does essentially is, then, to replace the subject term of ' The F is G ' by a conjunctive expression, one part of which expresses the property of being F and the other part of which expresses a proposition to the effect that F is not multiply applicable.

Two points remain to be dealt with. The first concerns the sense in which a usage is given when we say *the usage X*. If a usage were given merely by description as *the* usage which has X as its verbal component, then a statement of the form *The usage X is synonymous with the usage Y* could not express an analysis ; for we can perfectly well know a statement of this form to be true without having the least notion what the usages in question might be.[1] And since such an assertion could never express an analysis, we must ask what the facts are in this respect.

There can be no purely demonstrative reference to an object. If I simply point to a table and say ' that ', you may not know whether I mean *that table* or *that table-top* or *that surface* or *that texture* or *that sense-datum* or *that shade of colour* or the like. But notice the difference between the last two possibilities and the others. There the object demonstratively referred to is directly given, in the way in which a proposition expressed by a sentence is directly given to a hearer or reader who understands the sentence. And that is plainly the way in which a usage is given when it is referred to in the manner here employed ; if the usage is given by description, it is not merely given in that way. If S is a sentence, and I say

$$(\mathrm{E}p)\ (S \textit{ expresses } p\ \&\ p = \textit{the moon is round}),$$

[1] *Cf.* Moore, *loc. cit.*

I shall indeed be referring descriptively to a proposition; but the description I employ will involve an expression of the proposition described and cannot be understood by one who does not understand the sentence used to express the proposition.

The second point concerns the relation of formal analysis to what we may call conceptual analysis. The latter is not concerned with usages, but with the concepts and propositions expressed by verbal expressions. And here the verbal expression expressing the analysandum will not be synonymous with the verbal expression expressing the analysans. We require, in particular, that there shall be at least one concept which is a component of the analysans but not of the analysandum, and, further, that the analysans and the analysandum shall entail each other. Suppose a man to have worn all his life a peculiar pair of glasses, which enable him to distinguish objects orange in colour from other objects, but which render him unable to recognise an object as being red or as being yellow. Then he will be unable to understand the proposition *A thing would be orange in colour if and only if it was also intermediate in colour between red and yellow*, because he will not have formed all the relevant concepts. The concept of being red, for example, which is an ostensively defined concept, is a conscious habit of recognition, which is the habit of recognising any object, of whatever specific quality, as having a quality of the kind *red*; and he will not have formed that concept. The proposition here cited will therefore be a conceptual analysis of the property of being orange in colour.

C. H. LANGFORD.

A NOTE ON INDUCTION

THERE seem to be two fundamentally opposed attitudes which I, as an agent in this world, can adopt towards the question of the validity of inductive method. I can decide to act on the belief, or assumption, that ' particular matters of fact ' are, in general, amenable to inductive investigation ; or I can (theoretically) decide to act on the assumption that they are *not*. I may of course make mental reservations, or suspend judgement, about inductive procedures and steel my logical conscience neither to accept them nor reject them : but as an agent I must act as though I trusted them—or distrusted them.

Now suppose I decide to act on the first assumption, that inductive methods generally ' work '. By so acting—and believing—I am providing some evidence for the truth of the assumption, since inductive behaviour is itself one of those ' regularities ' in the world upon whose existence the possibility of drawing valid inductive conclusions depends. The actions of anyone who acts according to rules are (theoretically) assessable by inductive methods : and it is logically impossible for anyone to act inductively and not act according to (inductive) rules. From which it follows that my acting inductively is incompatible with the complete fallaciousness of inductive method. As long as I continue to trust in induction, so long will some applications of inductive rules be ' valid ' in the world : my actions guarantee the presence of at least one law-abiding element in the world. It seems almost to be a case of ' Believing makes it so '—or partly so.

Of course this argument does not show that men will in fact continue to act according to inductive rules : at most it proves that if they continue so, inductive methods cannot be wholly invalid.

Can we go on to prove that men could not distrust inductive methods unless such methods were partially inapplicable—inapplicable at any rate to their own non-inductive behaviour ? (By ' inapplicable ' I am not referring merely to ' practical ' difficulties about the making of inductions, but rather to the ' logical impossibility ' of making successful inductions in an utterly ' lawless ' world.)

One argument does claim to prove that distrust of induction would entail this ' inapplicability ' : for any state of affairs in which men abstained from acting and arguing inductively would itself ' justify ' such abstention, since this abstention would falsify one of the most fundamental and best-established inductions ever made—namely, that man is an ' inductive animal ' over wide sectors of his everyday experience and activity. " We know by induction that man will always rely on induction. If we don't know this—if it isn't true—so much the worse for induction in general. Would not the breakdown of this particular induction show that *no* inductions were reliable—that the whole inductive procedure was radically unsound?"

The objection to this argument is that it does not seem true to say that the failure of any particular inductive conclusion, however 'fundamental', entails the uselessness of inductive method in general, or even its inapplicability to the 'subject' of that particular induction. This would only be true, in the present case, if the following disjunction were true: either man behaves inductively or else his behaviour does not admit of inductive investigation. If this were so, any abstention from the 'inductive habit' would entail a corresponding increase of irregularity in human behaviour. But there are other ways of acting regularly, besides inductive ways. For instance, a man may habitually pray to God whenever he desires relief of his distress: and even though perhaps he has never known such prayers be answered in the past, he may continue to pray, and continue to expect relief. As long as he follows this rule—'Pray to God in time of trouble'—his behaviour in times of trouble is to that extent inductively calculable.

Of course, it may be denied that men have the freedom to choose whether to follow inductive method or some other procedure, at any rate where most of their actions are concerned. If this is true, the debate on induction becomes somewhat 'unreal', in the sense that whatever decision we may theoretically take about the validity of induction, this decision will not affect our conduct one way or the other. Suppose, for instance, we are 'bound' to act inductively, whatever logical sentence we may pass on such action: in that case we would do better to submit unprotestingly to necessity, getting what comfort we could from the reflection that the existence of this necessity is incompatible with the total invalidity of inductive procedure. In a completely 'lawless' world there wouldn't be any men capable of having neurotic anxieties about the 'problem of induction' and possible failures of inductive ventures: it is logically impossible that the nightmare picture of an utterly lawless world in which men with memories of the 'good old days' went on expecting food to nourish, fires to warm, should become a waking reality.

If, on the other hand, it is possible for men to choose between inductive procedure and other procedures, and to act on their choice: then whatever procedure they adopt, they will be making inductive rules applicable to their own performances, whether these be inductively-guided or not. Some may decide that crystal-gazing is a better method than induction; and as there are rules for crystal-gazing and a regular operational technique, by becoming crystal-gazers they will be demonstrating the impossibility of crystal-gazing being the only possible truth-attaining technique. Crystal-gazers give a logical boost to induction merely by being consistent gazers, without giving any such boost to their own predictive methods: these may come off, or not. All non-inductive procedures-according-to-rule invite the logical attention of Mill's men: whereas my following inductive rules does not prove anything about the 'validity' of the crystal or any other non-inductive technique; does not entail,

for instance, the 'gazability' of my future conduct through the crystal. It is logically possible that inductive rules should be the only rules whose application can bring information in advance of experience, logically impossible that any other set of applied rules should be the only set. In this respect, inductive rules seem to be in a stronger logical position than any rival set of rules. Crystal-gazers are under logical pressure to admit that inductive methods can sometimes be employed successfully (or could be, given sufficient skill on the part of the makers of inductions); Mill's men are under no such logical pressure to admit the partial validity of the arts of sorcery and magic.

If this is true, then anyone who is making a choice between inductive and non-inductive rules is presupposing the applicability of inductive rules as far as his own future conduct is concerned, whether he realises it or not. Having made this assumption, would he not be more consistent if he preferred to stake his future on induction, in default of definite evidence that this assumed regularity in his own behaviour would be the one exception in an otherwise lawless world? This would certainly be the more economical choice, involving the recognition of one set of rules instead of two or more sets—the inductive set plus any other sets adopted.

Of course the question may be put, 'Why make the reckless gamble of embracing any set of rules whatsoever? Why not act 'lawlessly' at all times?' Without bothering to quibble about whether such 'lawlessness' could be classed as action at all, as 'action' is used at present, one may perhaps side-step this question by repeating, with greater justification, an argument mentioned previously: any state of affairs in which men were able to 'act' lawlessly with such 'consistency' as to put them beyond the reach of induction would be one in which inductive procedure *had* 'broken down' over many acres of (hitherto) rich inductive soil; so without a special 'iron curtain' postulate of the division of the world into two halves, the human half and the non-human half, the lawless half and the law-abiding half, it would seem likely that when men find they can profess [1] lawlessness, and live up to their professions (however short their lives may be), any set of rules would be quite useless to them anyway.

R. J. SPILSBURY.

[1] Would not this 'professing' itself be a sort of performance-according-to-rule, which could be made the subject of inductive inquiry?

ON REPRESENTATIONS OF TIME AS "THE FOURTH DIMENSION" AND THEIR METAPHYSICAL INADEQUACY

In this note I propose to make some remarks about the representations of time as "the fourth dimension" and the disabling limitations to which they are necessarily exposed in the metaphysics of time. Despite Professor Broad's warning that we must not talk, or listen to, nonsense about time as "the fourth dimension of space", non-technical expositions of Relativity have been prone to represent the spatio-temporal relation between events as a sort of "distance" in four dimensions. The idea of time as "the fourth dimension" is older than Minkowski and at least as old as d'Alembert, Lagrange and Fechner. But it was Minkowski who in 1908 showed that the space-time of Special Relativity becomes formally equivalent to an isotropic four-dimensional Euclidean continuum if time is replaced by the imaginary co-ordinate $\tau = \sqrt{-1}\ ct$ where c is the velocity of light. In popular expositions of the theory it is usually assumed without argument that by dissecting the "objective" four-dimensional continuum we get "space-like" and "time-like" intervals. Minkowski unfortunately introduced $\sqrt{-1}$ as a "mystical" symbol for clothing his "world-postulate" [1] and the handling of the question by Jeans and Eddington has not been particularly helpful. We know that Jeans spoke of $\sqrt{-1}$ as a "weird" unit for measuring time [2] and that Eddington, while discouraging metaphysical speculation on its implications, ascribed to it the mysterious property of "turning time into space".[3] Most philosophers have been content to accept $\sqrt{-1}$ as an arbitrarily chosen "analytical device". It is sometimes said that time in space-time geometry "becomes Euclidean" (whatever that may mean) when multiplied by $\sqrt{-1}$.[4] Mr. Dunne has cast the glamour of his "Serialism" over Relativity by offering to interpret $\sqrt{-1}$ as an "operator" producing perpendicularity from a datum-line. He equates time-lapse with distance travelled along a fourth dimension of Euclidean space S_4 or T_1 and, to time the travelling, posits a time-axis T_2 orthogonal to T_1. Rotating T_2 through a

[1] *The Principle of Relativity*, Einstein and others (Eng. trans.), p. 88.

[2] *The Mysterious Universe* (1931), p. 110.

[3] MIND, N.S., Vol. XXIX, No. 116, p. 419; *The Mathematical Theory of Relativity*, p. 12; *Space, Time and Gravitation*, p. 48; *Relativity Theory of Protons and Electrons*, p. 83.

[4] See for instance F. W. Lanchester's *Relativity* (Constable & Co.), Addenda to Part I.

right angle is multiplying it by $\sqrt{-1}$.[1] That $\sqrt{-1}$ has not yet shed the mystical halo which Minkowski unwisely put round it must be evident from a recent statement by Mr. C. A. Richardson : " In spite of the fusion of space and time into the continuum of space-time, there persists a sense of the psychological difference between the perception of space and the perception of time. Corresponding to this psychological difference there is a difference between space and time directions in physical space-time signified by the occurrence in the equations of the symbol $\sqrt{-1}$." [2] Does this mean that the mathematical symbol is introduced to preserve a distinction felt to be psychologically or epistemologically important ?

In a paper and supplementary notes contributed to MIND [3] I have attempted to explain why $\sqrt{-1}$ enters into the Euclidean representation of the Minkowski continuum. If one sets out from a conventional Euclidean " map " of space-time representing time and a single dimension of space, it is not an arduous task to show that the imaginary isotropic lines with the slopes $+i$ and $-i$ (where $i = \sqrt{-1}$) have a role analogous to that of the light-tracks in the kinematic picture. I have suggested that the geometrical analogy offers a not implausible interpretation of the well-known " paradoxes " of Special Relativity, especially the zero intervals along the world-line of light.[4] A. A. Robb in his careful treatment of the problem has remarked that the conception of length breaks down partially, if not entirely, for " optical lines " in space-time geometry.[5] The analogy of isotropics in the Euclidean plane is not unprofitable in studying the kinematic picture.[6]

What philosophers must realise is that the alternative " geochronometries " developed in mathematical physics convey little or no information about the ultimate nature of time. Viewing the problem from an epistemological standpoint, we may even say that we get out of time what we put into it. Time regarded as a magnitude cannot be measured directly. The only procedure open to us is to choose a sequence the boundary events of which are asymmetrically related by before-and-after and between the successive

[1] J. W. Dunne, *The Serial Universe* (Faber), Chs. XIV and XV.

[2] *Happiness, Freedom and God* (Harrap & Co. Ltd., 1944), p. 46.

[3] Vol. XLVI, No. 182, pp. 159-179 ; No. 183, pp. 415-416 ; Vol. XLVII, No. 188, pp. 550-552.

[4] The puzzle has exercised more than one writer on Relativity. *Cf.* Russell, *The Analysis of Matter*, p. 70 ; G. N. Lewis, *The Anatomy of Science*, pp. 130-133 ; A. P. Ushenko, *The Philosophy of Relativity*, p. 69.

[5] *Geometry of Time and Space* (Cambridge), p. 3.

[6] C. W. O'Hara and D. R. Ward in their *Introduction to Projective Geometry* (Oxford, 1937, 12.6) have opined that the analogy points to an underlying identity of metrical structure. The asymptotes to Minkowski's hyperbola which plays the part of a circle in ordinary geometry correspond to the isotropics of Euclidean geometry treated projectively. *Cf. The Mathematical Gazette*, Vol. XX, p. 282.

members of which there is an interval of some other physical magnitude besides an interval of time. In experimental physics we habitually measure time-intervals in terms of space-intervals and yet we continue to suppose that the *kind* of interval measured by a clock is not the kind measured by a rod. The choice of space-intervals as the kind of intervals against which time can be graphed leads by the analytic extension of Euclidean space to Minkowski's four-dimensional isotropic continuum in which no "direction" can be picked out as fundamentally distinct from any other. The suggestion that $\sqrt{-1}$ in Minkowski's representation indicates the disparateness of time and space misses the point. The four-dimensional continuum formed of x, y, z and $\tau = \sqrt{-1}ct$ illustrates the failure of a metrical theory to preserve the conventional distinction between time and space accepted by experimental physics.

A pertinent question for contemporary metaphysics is whether the logical theory of a time-series constructed on the analogy of a one-dimensional number continuum is competent to deal with the more elusive, and possibly more essential, aspects of our experience of time. Is the relation of "betweenness" in linear time unambiguously implied by the reality of conscious selves and their states? The growing evidence for supernormal veridical precognition affords valid grounds for maintaining that such representations are not unequivocal. Recent research in the laboratory has shown that a subject can report correctly the chance result of a card shuffle a few seconds *before* the shuffle actually occurs doing this more often than he should if his "guesses" were random, although he does not appear to know consciously when he is making a significant response.[1] On psychological grounds it has been argued that a tendency to "spatio-temporal scatter" is characteristic of paranormal cognition, the displacement manifesting itself as "post-cognitive" or "precognitive" telepathy.[2] Speculation along these lines, however, does not enable us to grasp the *modus operandi* of precognition.[3] Without a far-reaching metaphysical theory of time the problem may not admit of a significant formulation. The attempts hitherto made to construct a metaphysic seem to be inspired, partly at any rate, by Mr. Dunne's theory. Professors Broad and Price have very tentatively put forward the hypothesis of a two-dimensional time to accommodate the facts of psychical research.[4] I venture to think that the postulation of

[1] *Vide* Professor Broad's discussion in *Philosophy*, Vol. XIX, No. 74, pp. 261-275, and Professor J. B. Rhine's guarded statement in *The Reach of the Mind* (New York, 1947), ch. 5.

[2] J. Ehrenwald, *Telepathy and Medical Psychology* (George Allen & Unwin, 1947), pp. 71-72. *Cf.* also Whately Carington, *Telepathy* (Methuen, 1945), ch. vi.

[3] *Cf.* G. N. M. Tyrrell, "The *modus operandi* of paranormal cognition" in the *Proceedings of the Society for Psychical Research*, May, 1947.

[4] "Knowledge and Foreknowledge", Aristotelian Society, Supplementary Volume XVI, pp. 177-245.

" extra dimensions of time " is hazardous without the most searching scrutiny of the concepts of " temporal betweenness ", before-and-after and " Becoming ". The analogy of " spatial betweenness " uncritically employed in popular discussions of the two-dimensional theory of time is apt to create difficulties. It should not be forgotten that there are limits to the usefulness of the concept of " betweenness " even in geometry. To take only one illustration of what I mean. A theorem transferable to complex space must not refer to one of three collinear points as " between " the others, since there is no corresponding relation between three arbitrarily chosen complex numbers. Assuming that for the special purpose of explaining precognition metaphysicians must work with a double series of terms, it by no means follows that every term of one series is in some sense or other " between " two terms in the other series. McTaggart's difficulties about the " A-characteristics " and the " B-characteristics " of events must be reconsidered from a new angle to-day. Behind the perplexities of psychical research lies the ultimate problem of the self and its relation to time. To pose the question whether the " time of consciousness " is " Euclidean ", " hyperbolic " or " elliptic " is premature ; for there remains the possibility that consciousness may have no " metric " uniquely associated with it.

C. T. K. CHARI.

THE RÔLE OF HYPOTHESIS IN SCIENTIFIC INVESTIGATION.

One of the most intriguing phases of scientific procedure is the nature, rôle and origin of the hypothesis. Actually if one examines any discussion of scientific method, he would find no hint at all concerning how to construct or find a "good" hypothesis. We are told that the hypothesis is necessary to "explain" facts and we are also told that the hypothesis is "suggested" by "relevant" or "significant" elements in the data. "Explanation" is a notoriously ambiguous term; while "relevance" requires ingenuity for its recognition. "Suggestion" seems to involve again an element more psychological than logical, for in order that data "suggest" the observer must be not merely open to suggestion but also able to *see* the suggestion. And not the least of the qualities which a good hypothesis must have is "fertility", *i.e.*, it must lead to predictions and it must open new vistas. Here too the fertility of a given hypothesis is a function of the ingenuity of the investigator's mind.

To illustrate briefly: The negative result of the Michelson-Morley experiment was a datum that did not fit into the Newtonian scheme. This meant that the Newtonian scheme of fundamental postulates and definitions did not imply the result of this experiment. More explicitly, according to classical physics, electro-magnetic and optical phenomena relative to the earth as a body of reference, should be influenced by the translational velocity of the earth; —the Michelson-Morley experiment showed they were not. In other words two difficulties were present: (1) the M.-M. experiment provided a fact that was not intelligible in the terms of classical physics, (2) the M.-M. experiment provided a fact that actually contradicted the implications of the hypotheses of classical physics. Either of these two conditions could have occurred alone, but their compresence complicated the problem. What hypothesis can be found to account for the following conditions:

(1) the velocity of light is constant *in vacuo*;

(2) the velocity of light is unaffected by the translational velocity of the body of reference? Note that the approach to a new hypothesis comes not by an accumulation of further facts, but by an attempt to find the conditions that will satisfy accepted and known facts. In other words, it is not a case of facts "suggesting" an hypothesis—but rather a case of finding a super-implicate. The procedure is more analogous to that involved in solving mathematical problems by "working backwards", *i.e.*, p_0 is true if P_1 is true, and P_1 is true if P_2 is true, etc. until a P_n is reached which is either a demonstrated theorem or a new axiom (in the case of physical

science—a new hypothesis). The mathematical formulation of the constancy of the velocity of light relative to an inertial system K is expressed by :

$$\Sigma(\Delta x_v)^2 - c^2\Delta t^2 = 0$$
$$\Sigma(\Delta x'_v)^2 - c^2\Delta t'^2 = 0\,;$$

while the second condition is given by the Lorentz Transformation equations. The fertility of the new relativity hypothesis was evidenced not merely in a host of new experimental problems, but also in its demand for a reformation of the basic notions of " time " and " distance " with the consequent rejection of " absolute space " and " absolute time ". But this much is a matter of history.[1]

Whenever one needs clean-cut illustrations of scientific method, he is sure to find them in the relatively simple phenomena studied by physics. If we need illustrations of how difficult it is to apply scientific method accurately, we must turn to other sciences, *e.g.*, biology or psychology.

To illustrate my theme, I shall consider the problem of cyclomorphosis in Daphnia.[2]

Daphnia is a microscopic organism that lives in fresh water. Among the many changes that occur in the life span of Daphnia there is one which is best shown in a sequence of generations. The sequence of generations, which occurs at intervals of weeks, is marked by a series of progressive modifications of the form of the head until the whole population is notably different from that of an ancestral one. The changes then occur in reverse order until a complete cycle is made. The difference between the summer form of the head of the Daphnia and its winter form is radical. It is as if a horse were to give birth to a colt with horns, which would give birth to a colt without horns and the cycle would be repeated. Such a phenomenon occurs among Daphnia and is known as " cyclomorphosis ". So radical is this change that earlier investigators assigned summer and winter generations to different species and even to different genera.

At this point a number of possible questions arise, each of which might lead to a different *type* of hypothesis and to subsequent directions of investigations. I shall simply list a few of these questions :

(1) What external factors (if any) are the stimuli for cyclomorphosis ?
(2) What internal conditions are the source of cyclomorphosis ?
(3) What functions (if any) does cyclomorphosis perform ?
(4) What structures are responsible for cyclomorphosis ? Etc.

[1] Students of logic interested in the development of the logic of the argument must be grateful to Einstein for the classic treatment in Albert Einstein, *The Meaning of Relativity*, reprinted in 1945 by the Princeton University Press.

[2] All data are taken from the paper by R. E. Coker, " The Problem of Cyclomorphosis in Daphnia ", *Quarterly Review of Biology*, vol. 14 (1939), pp. 137-148.

The answers that one proposes to these questions are the hypotheses advanced to explain the phenomena. It is to be noted that the direction of investigation is determined to a great degree by the questions which one asks in the attempt to explain cyclomorphosis, even though the questions are not completely independent and may lead to one another. Certainly hypotheses for questions (1), (2) and (3) will have bearing upon one another.

What determines which question is asked? The answer is perhaps startling when we consider the demands of objectivity, and raises again the problem of the meaning of 'objectivity'. What question is asked is determined by the categorical system which the scientist has adopted. If you will, call it his theory, or set of presuppositions, or his biasses. Two quotations, one from Darwin and one from a contemporary biologist, evidence what I mean.

"It is our habit, and one that might be called almost inveterate to assume for any phenomenon of organic life that it is adaptive, meaning that it has something to do with the welfare of the organism or that it has survival value. This seems on the face of it to be true of most, but probably not of all, structural or behavioral characters."[1]

This "inveterate habit" goes back to Darwin, at least; it must be recognised that this assumption already is an hypothesis, directing, so to speak, the investigator into specific channels in his search for subsidiary hypotheses. In a sense the statement "All organic phenomena are adaptive" may be viewed as a "law". Note, however, that if it is considered to be a "law", the questions concerning cyclomorphosis become questions concerning (1) the conditions to which cyclomorphosis is an adaptation (2) the adaptive functions which the changes perform. Questions concerning the physiology of the change, the hereditary nature of the change, etc. become subsidiary problems. The law implies at once that cyclomorphosis is adaptive and directs attention to factors which are adaptive. The law automatically excludes any investigations into elements clearly non-adaptive in nature.

At this point Darwin's remarks are appropriate:

"As far as I am able to judge, . . . , the conditions of life appear to act in two ways—directly on the whole organisation or on certain parts alone, and indirectly by affecting the reproductive system. With respect to the direct action . . . there are two factors, namely, the nature of the organism, and the nature of the conditions. The former seems to be much the more important . . .

With respect to what I have called the indirect action of changed conditions, namely through the reproductive system being affected, we may infer that variability is thus induced, partly from the fact of this system being extremely sensitive to any change in the conditions and partly from the similarity . . . between the variability which follows from the crossing of distinct species . . ."[2]

[1] R. E. Coker, *op. cit.*, p. 140.

[2] Charles Darwin, *The Origin of Species*, (Modern Library), N.Y., pp. 15-16.

It is to be noted that these remarks restrict even more the range of possibilities. The problems become more specified:

(1) What are the "conditions of life" which induce cyclomorphosis?

(2) Do these affect the whole organisation or only part?

(3) Do these affect the reproductive system?

(4) Is cyclomorphosis the result of the crossing of distinct species?

The second and third questions would await for their answer, the solution of the first question. Question 4, on the other hand, could lead to a denial of the hypothesis of adaptation. If two distinct species in the course of events crossed and the helmet development resulted, it is possible that the helmet had no adaptive value at all. The entire problem would then become an historical one. But the "inveterate habit" of assuming an adaptive function would tend to keep investigators from this assumption.

(Parenthetically, it is remarked that Darwin's words as quoted makes additional tacit presuppositions; for example, it seems to be implied that acquired characteristics which are successful adaptations are inherited.)

The fact that Daphnia resides in water leads to the next most obvious guess concerning the utility of the helmet. The argument might be paraphrased as follows: The conditions of life induce cyclomorphosis (hypothesis); The condition of life of the Daphnia is swimming (observation); Swimming induces cyclomorphosis. This, however, is a rather insipid conclusion, since the Daphnia swims or floats always. So the argument continues taking into account the seasonal character of cyclomorphosis: In the summer the water is warmer and lighter than in the winter; when the water is warmer and lighter more resistance to sinking is needed; the helmet offers such resistance—the helmet develops in order to aid the plankton to stay afloat in the summer. Such as matter of fact was the theory of a biologist, Wesenberg-Lund, proposed in 1900. Notice the air of finality and yet the sense of inconclusiveness about this. The hypothesis explains the known facts, it is empirically verifiable, it makes possible a limited amount of prediction—but it seems also to block further investigation unless one approaches the problem negatively.

The argument can now be phrased as follows:

(e_1) All organic phenomena are adaptive.
(f_1) The "helmet" is an organic phenomenon.
(c_1) ∴ The helmet is an adaptive phenomenon.
(f_2) Protuberances are aids to adaptation in water.
(c_2) ∴ The helmet is an aid to adaptation in water.

Note, however, that f_2 is not accurate. It should read "Protuberances are sometimes aids to adaptation". If this is recognised further testing is needed. Is this a case where the protuberance does aid floatation? Wolterek (1909, etc.) showed by an analysis of the

mechanism of locomotion that the helmets serve rather as directive and stabilising surfaces than as floatation aids. This line of investigation would seem to be closed. The helmet is seen to be an adaptive mechanism whose function is to aid the plankton in guiding the active movement of the animal.

In order to achieve further significant results some modification of the hypothesis becomes necessary. In place of the problem "What function does the helmet serve?", one can ask, "What stimuli bring about the development of the helmet?" At first glance this appears to be the first question asked differently—and to some extent it is, since the function served is relative to the stimulating conditions present. But note that the question might in its present form imply a non-functional approach. It is now possible, *e.g.*, to consider purely chemical stimuli that cause helmet development without any implication of adaptation whatever. As Coker points out (p. 143), the helmet might have utility after its development yet not have been developed to meet a need. In other words, the hypothesis (or law) "all organic phenomena are adaptive" is denied. With this denial there is the assertion "some organic phenomena are not adaptive", and new directions for investigation are opened—not the least of which is a consideration of the meaning of "adaptive". When is an organic phenomenon adaptive and when is it not? Does the fact that below 11° C. only round heads and above 15° C. only pointed heads are obtained indicate a non-adaptive characteristic? The hypothesis, in other words, defines clean-cut directions for investigation if the terms that occur in it are well-defined and distinct. No amount of observational or experimental data can be decisive unless and until terms are well-defined. Even the temperature variation just noted can be taken as evidence of (1) a chemical reaction, (2) the work of an organisator (Spemann), (3) the work of an entelechy (Driesch). Coker points out that the "influence of temperature acting directly upon the embryo . . . governs the form of the head of the newborn" (p. 146). The three alternatives are still possible in embryology.

Certain more or less obvious conclusions may be stated:

(1) There seem to be two levels of hypotheses. These are illustrated by the "law"—all organic phenomena are adaptive; and by the more specific statement—the helmet is induced by temperature changes. The former is usually part of the "theory".

(2) The meaning of subordinate hypotheses is determined by the "theory".

(3) Both levels of hypotheses if rigidly adhered to may lead to consistent results but will lead only to certain types of results.

(4) The "theory" may force an interpretation on data, which makes those data always conform to the theory. This may mean that no crucial datum is even possible.

(5) The only therapeutic techniques for the prevention of

theoretico-sclerosis is the deliberate adoption of contradictory theories as intellectual experiments.

And finally,

(6) The criteria for the selection of an hypothesis must include pragmatic considerations.

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VI.—CRITICAL NOTICES

Meaning and Necessity: A Study in Semantics and Modal Logic. By RUDOLF CARNAP. Chicago, University of Chicago Press; London, Cambridge University Press, 1947. Pp. viii + 210. 27s. 6d.

THE main purpose of this book is the development of what Carnap calls 'a new method for the semantical analysis of meaning'. The two central concepts of the method are those of *extension* and *intension*: according to Carnap every 'designator' (for instance, a predicate expression, an individual description, or a sentence) possesses an extension (sometimes called 'denotation') and an intension (sometimes called 'meaning'). Two designators are said to have the same extension if and only if they are (materially) equivalent, and to have the same intension if and only if they are L-equivalent, that is, roughly, strictly equivalent in Lewis's sense. This does not, of course, define the terms 'extension' and 'intension': as Carnap points out (p. 23), we have so far defined only the phrases 'having the same extension', and 'having the same intension'. To talk about extensions and intensions themselves, Carnap says, we have 'to look for entities, or at least for phrases apparently referring to entities, which can be assigned to designators in accordance with these definitions' (p. 23). As a result of this process of 'looking for entities' satisfying the definitions, Carnap 'finds' that the extension of a predicator is a class and its intension a property (pp. 16-22); next, the extension of a sentence is said to be its truth-value and its intension a proposition (pp. 25-32); and finally, the extension of an individual expression is the individual to which the expression 'refers', and its intension is 'a concept of a new kind', a concept which Carnap calls 'an individual concept' (pp. 39-42). To illustrate the last two points, the extension of the sentence 'Scott is human' is 'the truth', and its intension is the proposition that Scott is human; the extension of the name 'Walter Scott' is the individual (*i.e.*, the man) Walter Scott, and its intension is the individual *concept* Walter Scott. So far Carnap's conclusions resemble very closely those of Frege in his article 'Ueber Sinn und Bedeutung', apart from the fact that the introduction of 'individual concepts' is Carnap's own. But Carnap believes his method to be very different from Frege's, and to bring out what would appear to be at least one of the main differences, let us consider the following two sentences: (1) 'Scott is human', and (2) 'John believes that Scott is human'. With regard to the first sentence there is no difference between Carnap and Frege, apart from a difference in terminology: the extension (Frege's 'Bedeutung') of (1) is a truth-value; its intension (Frege's 'Sinn') is the

proposition that Scott is human. But with regard to the second sentence, Carnap claims, his own conclusions are very different from those of Frege: according to Carnap, the extension of (1) within the context (2) is still a truth-value, and its intension is still the proposition that Scott is human—in other words, the extension of (1) is precisely the same in both cases, and so is its intension. But according to Frege, the denotation ('Bedeutung') of (1) within the context (2) is not a truth-value, that is, not its ordinary ('gewöhnlich') denotation, but the proposition that Scott is human. In other words, according to Frege the ordinary sense ('Sinn') of the sentence 'Scott is human' becomes its denotation when the sentence occurs in an oblique ('ungerade') context. Similarly, the sense of (1) within the context (2) is no longer the proposition that Scott is human, but something else: Frege, Carnap says (p. 129), introduces 'a third entity as the oblique sense', although 'it seems that [he] nowhere explains in more customary terms what this third entity is'. And indeed Carnap never suggests what this 'third entity' of Frege could possibly be: a person who has not read Frege's article might well get the impression that Frege never goes into the matter at all. Yet he does, and the truth seems to me to be that Carnap has failed to understand him. On page 37 of his article Frege says, 'Zu den mit "dass" eingeleiteten abstracten Nennsätzen gehört auch die ungerade Rede, von der wir gesehen haben, dass in ihr die Wörter ihre ungerade Bedeutung haben, welche mit dem übereinstimmt, was gewöhnlich ihr Sinn ist. In diesem Falle hat also der Nebensatz als Bedeutung einen Gedanken, keinen Wahrheitswerth; als Sinn keinen Gedanken, sondern den Sinn der Worte "der Gedanke, dass . . .", welcher nur Theil des Gedankens des ganzen Satzgefüges ist.' In other words, according to Frege the oblique sense of (1), *e.g.*, the sense of (1) within the context (2), is not the proposition that Scott is human, but the sense of the words 'the proposition that Scott is human'. We shall see presently why Carnap has failed to understand Frege; for the moment let us see what are the consequences of attributing to (1) within the context (2) its ordinary denotation (extension). Compare the sentence (1), that is, the sentence 'Scott is human', with the sentence (3) 'Scott is featherless biped'. The denotation of (3) is, of course, precisely the same as that of (1); but now it seems obvious that the denotation of a compound expression depends solely on the denotation of its constituent expressions (*i.e.*, of those of its constituent expressions which do have denotation); and from this it follows that the denotation of the sentence 'John believes that Scott is human' is the same as the denotation of the sentence (4) 'John believes that Scott is a featherless biped'. It is clear, however, that this need not be so: it may be true that John believes that Scott is human and yet false that he believes that Scott is a featherless biped. This is one instance of what Carnap calls 'the antinomy of the name relation' (pp. 133-136); and his 'new method

for the semantical analysis of meaning ' must, of course, be capable of solving it. Frege's solution is implicit in my account of his views : he would say that, just as in the context (2) the sentence (1) has its oblique and not its ordinary denotation, so also in the context (4) the sentence (3) has its oblique and not its ordinary denotation ; and as the oblique denotation of (3) is, of course, different from the oblique denotation of (1), no ' antinomy ' arises. This is Frege's solution ; what is Carnap's ? So far as I can understand Carnap's discussion, it is briefly this. The principle which, as I have just said, seems an obvious one is in fact, it would appear, untrue. Carnap calls this principle (which he expresses in a slightly different way) ' Frege's first principle of interchangeability ' ; and although, for reasons which I shall discuss presently, he doesn't claim that the principle is untrue, he tells us that *his* method, that is, the method of extension and intension, does not employ it. *His* method, like Frege's, has also two principles of interchangeability (for it may be added that Frege has also another principle, namely, that the *sense* of a compound expression is a function of the senses of the expressions occurring in it), but they are different from Frege's : Carnap's first principle (p. 51), which is the one that concerns us here, allows for substitution of (materially) equivalent expressions, that is, expressions having the same extension, only in cases where these expressions *occur within an extensional context ;* and he has so defined this phrase that the sentence (1), as it occurs within the context (2), does *not* occur within an extensional context. (And similarly, the sentence (3), as it occurs within the context (4), does not occur within an extensional context). For, to quote Carnap, ' We say that a sentence is *extensional* with respect to an expression occurring in it or that the expression occurs in the sentence *within an extensional context*, if the expression is interchangeable at this place with every other expression equivalent to it '. (p. 46). This, then, appears to be Carnap's solution of the antinomy ; and I think it must be admitted that if we do restrict Frege's principle in this way, no antinomy will arise. But surely this solution, even if it be the right one, is very unsatisfactory, at least *prima facie.* For we can well ask, ' If the extension of (1) within the context (2) is its ordinary extension, *why* can't we substitute for it any other sentence having the same extension ' ? Is this question meaningless ? Is it, so to speak, just a fundamental fact that in some contexts such substitution is permissible whilst in other contexts it is not ? Or is it possible to define these different kinds of context in a way which would not involve reference to the notion of interchangeability ? I am afraid Carnap doesn't answer these questions : in fact he doesn't even discuss them. And I do not therefore think that his solution, though technically it works, is philosophically satisfactory. To put the matter in another way. Frege's first principle, which seems an obvious one, combined with the view that the extension of a designator is always the same, leads to a contradiction. Carnap

therefore restricts the principle; but he does not, so far as I can see, give any *independent* support in favour of this restriction. I am inclined to say that whilst Frege's solution, whether it be right or wrong, gives an *explanation* of the difficulty, Carnap's solution does not. It seems to be a purely arbitrary one.

I have pointed out that Carnap does not say that Frege's first principle is untrue; but it seems to me that his solution does amount to maintaining that the principle must be restricted, and if it must be restricted, it is presumably untrue when used without restriction. However, Carnap believes that the difference between Frege's method and his own is not a difference of opinion. 'It is not the case', he says, 'that there is one question to which different and incompatible answers have been given. There are two questions, and more precisely, these are not even theoretical questions but merely practical aims. While the general aim is the same, namely, the construction of a pair of concepts suitable as instruments for semantical analysis, the specific aims are different. Frege tries to achieve the general aim by an explication of one pair of concepts, I by the explication of another pair' (pp. 127-128). But these modest and pacificatory remarks are, I fear, of very little use unless we are told *exactly what concepts* are supposed to be 'explicated' by Frege and *exactly what concepts* are supposed to be 'explicated' by Carnap. And this, it seems to me, Carnap has lamentably failed to do. He tells us that we find in 'the historical development' *two* pairs of correlated concepts appearing in various forms. These pairs, he says, are 'closely related' to each other—so closely, in fact, that they 'may sometimes even merge' (p. 126). And the closeness of this relation is also shown by the fact that 'it seems that the second concept of this [second] pair is very similar to the second in the first pair; for both of them the term 'meaning' is sometimes used' (p. 127). The first two concepts (Carnap's alleged 'explicanda') are the concepts of extension or denotation ('in the sense of J. S. Mill'), and intension or comprehension or meaning or connotation. These, we are told, we often find in traditional logic. The second pair of concepts (Frege's alleged 'explicanda') 'starts with the name-relation'. For instance, the expression 'Walter Scott' is ordinarily said to be a name of the man Walter Scott. Logicians, Carnap says, extend the application of this relation and even construe expressions of another than the individual type as names; and, he goes on, 'with respect to any expression regarded as a name, a distinction is made between that entity whose name the expression is and the meaning or sense of the expression' (p. 127). But are there here *two* pairs of concepts? Has not Carnap simply used different terminologies to state the same distinction? I do *not* believe that we have here two pairs of concepts, and I think there is no doubt that Carnap has done nothing at all to *show* that we do. Yet this is the basis of his claim that there is no difference of opinion between him and Frege!

Incidentally, it seems to me quite unclear what ' explication ' is really supposed to mean. What exactly is the relation which must hold between an ' explicandum ' and its ' explicatum ' ? ' Generally speaking ', we are told on page 8, ' it is not required that an explicatum have, as nearly as possible, the same meaning as the explicandum ; it should, however, correspond to the explicandum in such a way that it can be used instead of the latter '. This seems to me to be exceedingly vague ; yet the problem is surely of great importance for Carnap's work : for how are we to tell whether, for instance, Carnap's concepts of extension and intension do or do not ' explicate ' some more familiar concepts, unless we know what conditions an ' explicatum ' must satisfy ? And how does Carnap use the word ' meaning ' when he says that an explicatum need not have exactly the same meaning as the explicandum ? Is *this* use of ' meaning ' analysed anywhere in the book ? Or would Carnap say that this use of the word involves ' psychological factors ' and relegate its analysis to some non-existent science of ' pragmatics ' ?

Let us now return to our sentences (1) and (2). I have said earlier on that according to Frege the sense of (1), as the sentence occurs within the context (2), is the sense of the words ' the proposition that Scott is human '. This, I think, is clear from the passage which I have quoted from Frege's article. Why, then, does Carnap say that Frege nowhere explains in more customary terms what is the ' entity ' which he considers to be the oblique sense of a sentence ? I think the reason is that Carnap makes what seems to me to be a dreadful confusion. He seems to confuse the meaning of the *sentence* ' Scott is human ' with the meaning of the *phrase* ' the proposition that Scott is human '. For instance, on pp. 155-156 he tells us that in his ' neutral ' language M′, the ' neutral ' formulation ' Scott is human ' replaces both the sentence ' Scott is human ' and the ' non-neutral ' phrase ' the proposition that Scott is human '. But how can anything ' replace ' both the sentence and the phrase in question unless these have the same meaning ? Yet isn't it quite clear that they do not ? I think it is, and in order to see this it is enough to notice that whilst the sentence ' Scott is human ' expresses a proposition, the phrase ' the proposition that Scott is human ' does not express a proposition at all. The phrase ' the proposition that Scott is human ' is not, of course, a sentence. And I fear that, in addition to *this* confusion, Carnap is also guilty of another confusion. He seems to identify the meaning of the sentence ' Scott is human ' with the *fact* that Scott is human. Hence, we get the incredible result that the meaning of the phrase ' the proposition that Scott is human ' is the fact that Scott is human ! I have just said that to identify the proposition that Scott is human with the fact that Scott is human is to be guilty of a confusion. Carnap, however, does not think so. He tells us (p. 28) that the word ' fact ' in its ordinary use is ' rather vague and ambiguous ', and that hence there is some freedom of choice left as to

how to 'explicate' it. And he is inclined to think that it would not deviate 'too much' from customary usage if we were to 'explicate' the term 'fact' as referring to a certain kind of proposition. I fear, however, that to identify the proposition that Scott is human with the fact that Scott is human, *is* fallacious. For surely the fact that Scott is human is identical with the fact that the proposition that Scott is human is true; hence, if the fact that Scott is human is identical with the proposition that Scott is human, it follows that the proposition that Scott is human is identical with the fact that the proposition that Scott is human is true. And does Carnap really think that the proposition that Scott is human is identical with the fact that the proposition that Scott is human is true?

To explain our use of such terms as 'property', 'concept' and 'proposition' is indeed a task of the greatest philosophical importance. But Carnap's naïve explanations (*e.g.*, that the word 'concept' must not be understood in a 'mental' sense, but as referring to 'something objective that is found in nature') are no use at all. And it doesn't help much to be told that if one finds Carnap's explanations more puzzling than clarifying one may simply disregard them.

Carnap's treatment of a number of other problems seems to me to be also very unsatisfactory; but I cannot discuss here all these problems, and I shall mention only a few. The first is that of the analysis of sentences about beliefs; the second is Carnap's solution of Moore's paradox of analysis; the third concerns the possibility of an extensional metalanguage for semantics. I shall take them in order.

Let us consider the sentence (*a*) 'John believes that there is nobody who is a brother and is not male'. Carnap points out (I am applying Carnap's discussion to this particular example which is mine) that it won't do to say that (*a*) means the same as, or can be 'interpreted' by, (*b*) 'John is disposed to an affirmative response to the sentence 'there is nobody who is a brother and is not male'.' And this, of course, is perfectly correct: it is not necessary for the truth of (*a*)—or, as I should say, for the truth of the proposition expressed by (*a*)—that John should understand the English sentence in question. He therefore suggests the following 'more cautious' analyses, namely, (1) that (*a*) means the same as (*c*) 'John is disposed to an affirmative response to some sentence in some language, which is L-equivalent to 'there is nobody who is a brother and is not male'', and (2) that (*a*) means the same as (*d*) 'John is disposed to an affirmative response to some sentence in some language which expresses the proposition that there is nobody who is a brother and is not male'. (Pp. 53-55). Now the meaning of 'being disposed to an affirmative response' is not, of course, very clear; nor do I understand at all clearly what Carnap means by saying that he takes the word 'belief' in the sense of 'expressible belief'; but quite apart from this he himself points out that these suggestions won't do.

For he so defines L-equivalence that any two sentences each of which expresses a necessary proposition are L-equivalent; hence it is obvious that (*c*) may be true and yet (*a*) false. Similarly, he tells us on page 27 that he has 'decided' to take L-equivalence as the condition under which two sentences express the same proposition; and hence it is also obvious that (*d*) may be true and yet (*a*) false. Incidentally, Carnap can, of course, 'decide' to take as an identity condition for propositions anything he pleases; but in view of his actual decision, what does he take his concept to be 'explicating'? It seems to me that there is *no* ordinary use of 'expressing the same proposition' in which the sentence 'there is nobody who is a brother and is not male' can be said to express the same proposition as the sentence 'there is nothing which is red and is not coloured'. However, in view of Carnap's decisions neither (*c*) nor (*d*) will do as an analysis (or 'interpretation') of (*a*). But he introduces another concept with the help of which a correct interpretation of (*a*) can, he thinks, be given. The concept in question is that of intensional isomorphism (pp. 56-59). It would take too much space to explain this concept in detail; but the main idea can be made fairly clear by applying Carnap's definition to a few examples. Thus the sentence 'John is a brother' and the sentence 'John is a male sibling', though they are L-equivalent, are *not* intensionally isomorphic. Similarly, the sentences 'there is nobody who is a brother and is not male' and 'there is nothing which is red and is not coloured', though L-equivalent, are again *not* intensionally isomorphic. On the other hand, the sentences 'London is large' and 'London ist gross' *are* intensionally isomorphic, and so are the two expressions 'The King of France' and 'Le roi de France'. I think the main point is now fairly clear; but perhaps it may be worth while to quote a part of Carnap's own summary. 'If two sentences are built in the same way out of designators (or designator matrices) such that any two corresponding designators are L-equivalent, then we say that the two sentences are *intensionally isomorphic*.' Carnap's interpretation of the belief-sentence (*a*) can now be given. It is this. The sentence (*a*) can be interpreted by the following 'semantical' sentence (*e*) 'There is a sentence $\mathfrak{S}_i$ in a semantical system S′ such that (i) $\mathfrak{S}_i$ is intensionally isomorphic to 'there is nobody who is a brother and is not male' and (ii) John is disposed to an affirmative response to $\mathfrak{S}_i$.' (P. 62). But this, I think, won't do either. For suppose that John says, 'I believe that Henry is a male sibling', and that I, in conversation with another person, say 'John believes that Henry is a brother'. If John's assertion is true, doesn't it follow that mine is also true? I think there is at least one common use of 'believe' in which this does follow; and, if so, it is not necessary for the truth of the proposition expressed by the sentence 'John believes that Henry is a brother,' in this use of 'believe', that John should be 'disposed to give an affirmative response' to the sentence 'Henry is a

brother ': it is enough that he should be so disposed towards the sentence ' Henry is a male sibling '. In other words, whilst (*e*) is a sufficient condition for (*a*), it is not a necessary condition. Would Carnap maintain that in this case we are not taking ' belief ' in the sense of ' expressible belief '? Carnap says, ' The analysis here proposed is not yet a complete solution, but it may perhaps be regarded as a first step. What remains to be done is, first, a refinement of the analysis in terms of linguistic reactions here given and, further, an analysis in terms of dispositions to non-linguistic behavior ' (p. 62). I fear that if what I have just said is correct, Carnap's analysis cannot be regarded as even a first step—at least not in the sense he intended it to be. And much more ' remains to be done ' than Carnap seems to realise.

I have gone into this matter in some detail partly because of its intrinsic interest and partly because it illustrates the inadequacy of Carnap's new conceptual apparatus. He has introduced two concepts, L-equivalence and intensional isomorphism. The first is too weak for the solution of the problem under discussion, the second is, I think, too strong. He has failed to introduce a concept with the help of which the problem can, perhaps, be solved (or advanced), namely, a concept which would ' explicate ' at least one common and important use of ' being the same proposition '. I mean that use in which the proposition that Henry is a brother *is* the same as the proposition that Henry is a male sibling, but in which the proposition that there is nobody who is a brother and is not male is *not* the same as the proposition that there is nothing which is red and is not coloured. And in which, also, the proposition that the number of inhabitants of Chicago is greater than three million is *not* the same as the proposition that the number of inhabitants of Chicago is greater than $2^6 \times 3 \times 5^6$.

Carnap says that an interpretation analogous to (*e*) also holds for ' other sentences containing psychological terms about knowledge, doubt, hope, fear, astonishment, etc., with ' that '—clauses '. But if we take the sentence ' John knows that Henry is a brother ', it is even clearer that Carnap's analysis won't do. For here there is, I think, no doubt that, in any ordinary use of ' know ', if John knows that Henry is a male sibling, it follows that John knows that Henry is a brother. Yet the sentences ' Henry is a brother ' and ' Henry is a male sibling ' are not intensionally isomorphic.

Let us now consider sentences of the form ' John is committed to maintaining that . . .'. What would be Carnap's interpretation of such sentences? So far as I can see, he would be unable, with the concepts at his disposal, to point out any difference between sentences of this form and sentences of the form ' John knows that . . .'. For in either case if the subsentence '. . .' is replaced by a sentence L-equivalent to it, the truth-value of the whole sentence may change. Yet there *is* an important difference between sentences of these two forms. For contrast the sentence (α) ' John

knows that the number of inhabitants of Chicago is greater than three million ' with the sentence (β) ' John is committed to maintaining that the number of inhabitants of Chicago is greater than three million '. If in the former sentence we substitute for the subsentence ' the number of inhabitants of Chicago is greater than three million ', the sentence ' the number of inhabitants of Chicago is greater than $2^6 \times 3 \times 5^6$ ', the truth-value of the whole sentence (α) may change ; but the same substitution will not change the truth-value of the sentence (β). In other words, in sentences of the form ' John is committed to maintaining that . . .' we can substitute for '. . .' any sentence expressing a proposition which is *logically* equivalent to the proposition expressed by '. . .' (or which is entailed by it) ; but we can't do this in sentences of the other form. In neither case, however, can we substitute for '. . .' a sentence expressing a proposition which is merely *strictly* equivalent to that expressed by '. . .', or which is merely *strictly* implied by it. For instance, if in ' John is committed to maintaining that there is nobody who is a brother and is not male ', we substitute for the sentence which follows the word ' that ', the L-equivalent sentence ' there is nothing which is red and is not coloured ', the truth-value of the whole sentence may change. (There is much more to be said about sentences of this form, but I can't enter into this here). I think therefore that Carnap ought to have introduced two more concepts, namely the concepts of entailment and of logical equivalence (mutual entailment) ; or he should have defined L-implication and L-equivalence in a different way. And perhaps (I do not know) the concept of mutual entailment might have helped him to explain his notion of explication.

Let us now turn to Carnap's alleged solution of Moore's paradox of analysis. I shall state the paradox in a form slightly different from that which is used by Carnap in order to bring out more easily the unsatisfactoriness of his ' solution '. Consider the following two propositions, (I) ' The proposition that John is a brother is the same as the proposition that John is a male sibling ', and (II) ' The proposition that John is a brother is the same as the proposition that John is a brother '. (I) is clearly true ; but if (I) is true, and if, further, (I) entails nothing about the *sentence* ' John is a brother ' and the *sentence* ' John is a male sibling ' (this important condition Carnap oddly neglects), then it seems to follow that (I) and (II) are one and the same proposition. Yet it seems clearly to be the case that (I) and (II) are *not* one and the same proposition. This, briefly, is the paradox. What is Carnap's solution ? If I have understood him correctly, it is this. (I) and (II) *are* one and the same proposition : for the sentences which I have used to express (I) and (II) are L-equivalent ; but at the same time there is an ' important difference in meaning ' between these sentences since they are not intensionally isomorphic. I am afraid that this seems to me to be no solution at all : it seems to me that Carnap's concepts, far from

enabling him to solve paradox, do not enable him even to state it correctly. For consider the two propositions, (A) ' John is a brother ', and (B) ' John is a male sibling '. According to Carnap, the relation between (A) and (B) is precisely the same as that between (I) and (II) : here again (A) and (B) are one and the same proposition in the sense that the sentences which I have used to express them are L-equivalent; but there is an ' important difference in meaning ' between these sentences since they are not intensionally isomorphic. But if Carnap's concepts do not enable him to point out any difference in the relationship between (A) and (B) and that between (I) and (II), then his concepts do not enable him to state correctly Moore's paradox. For the paradox arises precisely from noticing that there is a sense in which (A) and (B) *are* one and the same proposition, but in which (I) and (II) are *not* one and the same proposition. (In fact there seems to be *no* sense in which they are). Yet if (A) and (B) are, in this sense, one and the same proposition, and if the assumption I have mentioned above is also correct, it seems to follow that, in the same sense, (I) and (II) are also one and the same proposition. Carnap complains that none of the writers who have discussed the paradox (including Moore), has stated his criterion for the identity of ' meaning ', ' statement ', or ' proposition ' ; and he adds ' this seems the chief cause for the inconclusiveness of the whole discussion '. But these remarks seem to me to involve a terrible misunderstanding. For what Moore's paradox does is to bring out an apparent contradiction in the *requirements* for a correct definition, or a correct explanation of our use, of sentences of the form ' the proposition that . . . is the same as the proposition that – – –'. In other words, the paradox concerns an apparent contradiction in the ' adequacy-conditions ' which such a definition (or explanation) must satisfy. And in fact the position is a little more subtle than this, because it is not the case that there is an apparent contradiction between *different* requirements, but that the requirement which such a definition *must* satisfy is one which, it seems, it *cannot* satisfy : namely, the requirement that the definition be such as to enable us to say that (A) is the same proposition as (B), but that (I) is not the same proposition as (II). And in order to get rid of this apparent contradiction it would be necessary to explain our use of such phrases as ' the proposition that John is a brother ' and ' the proposition that John is a male sibling ', as they occur in the sentence ' the proposition that John is a brother is the same as the proposition that John is a male sibling '. And this Carnap has failed to do if only because of the confusions which I have already mentioned.

I must now bring this review to an end, and I can discuss only very briefly the remaining point. In his last chapter but one Carnap deals with the possibility of an extensional metalanguage for semantics (pp. 168-172), and there is one point in this discussion which has puzzled me a good deal. Carnap is inclined to believe

(though he agrees that the problem requires further investigation) that it is possible to give a complete semantical description even of an intensional object language in an extensional metalanguage. And he explains that a semantical description of an object language is complete ' if it, given as the only information about the language, enables us to understand every sentence of the language ' (p. 168). The principal difficulty in this undertaking concerns the rules of designation. Carnap points out that in his extensional metalanguage it is true both that ' H ' designates Human and that ' H ' designates Featherless Biped. In view of this fact, he says, it might seem at first glance as though it were impossible to give in this metalanguage ' the information about the meaning intended for ' H ' '. He believes, however, that this is not impossible. He says that in his metalanguage we ' lay down ' that ' H ' designates Human among the rules of designation for the object language in question ; and he points out that, in this case, whilst it is also true that ' H ' designates Featherless Biped, the latter statement is fundamentally different from the former. For the latter statement is then neither a semantical rule nor derivable from semantical rules alone. I find all this very puzzling. Of course, what Carnap says is quite true ; but suppose we reverse the position by ' laying down ' that ' H ' designates Featherless Biped. In this case we should fail to give in the metalanguage ' the information about the meaning intended for ' H ' '. But what is there to prevent us from ' laying down ' that ' H ' designates Featherless Biped, if this is true ? No doubt what will in fact prevent us from doing so is our understanding of the object language in question. But in this case, even if the metalanguage, once constructed, can be said to give us a complete semantical description (in the sense explained) of our object language, aren't we, in constructing it, already assuming an ' interpretation ' of the object language which *cannot* be formulated in purely extensional terms ? However, I may have simply misunderstood the problem, and I fear that what I have just said is probably nonsense. Yet I can't help feeling that there is a difficulty here which has not been solved.

The book deals with a great many other topics, and there can be no doubt that it will give rise to much discussion.

C. Lewy.

Nicolaus of Autrecourt. A Study in 14th Century Thought. By Julius Rudolph Weinberg. Princeton University Press, for the University of Cincinnati. (London : Geoffrey Cumberlege.) Pp. x, 242. 21s. net.

Both for its own sake and because of its place in the history of thought, the system of Nicolaus de Ultricuria deserves study ; Dr. Weinberg has skilfully reconstructed it from the extant fragments,

and we may well be glad that so much has survived the flames to which Nicolaus's works were condemned. Dr. Weinberg's exposition is clear and pleasantly written, and sustains the reader's interest throughout. He obviously thinks that what medieval philosophies have to say can be stated in good modern English ; unlike some writers on this subject, he neither makes frequent quotations from the Latin nor fills his text with Latin technical terms.

To show what this book is about, I will begin by giving a brief account of Nicolaus's main lines of thought ; I am here mainly making a *précis* of Dr. Weinberg's own synthesis.

Nicolaus's system divides naturally into two parts : first, the destructive criticism, which removes all *certainty* except as regards immediate experience and the law of non-contradiction ; secondly, the reconstruction of a *probable* view on the assumption of a good and well-ordered universe, which is held to involve that ' whatever is clearly and evidently conceived is true ' (p. 186). Descartes is the obvious modern parallel ; surprisingly, there is only one passing mention of him (p. 28).

From the extracts Dr. Weinberg gives (*e.g.*, on p. 88) it is clear that Nicolaus and many of his contemporaries shared the view that intuitive cognitions are ' absolute entities ', not constituted by a relation to any object of cognition. (The appearance here of this view has a great historical importance, which Dr. Weinberg does not bring out ; it is foreign to Plato and Aristotle and the best thought of the thirteenth century, but its influence in modern philosophy, from Descartes to Russell, has been enormous.) This is presupposed on both sides in Nicolaus's correspondence with Bernard of Arezzo. Bernard had argued that any intuitive cognition could logically be just what it is—God could so make it—even if its object did not exist. The result, as Nicolaus points out, is complete scepticism ; Bernard would not even have the right to say ' I believe ' or ' I am in pain ', since his intuitions of such states need not have any real belief or pain answering to them. To escape this difficulty, Nicolaus asserts that some cognitions (sensory cognitions in particular) are cognitions of *themselves*.

He relates the certainty of these cognitions to the law of non-contradiction. Only this law and its consequences can be really certain ; in all other cases it is logically possible—involves no contradiction—that the alleged certainty should exist as a psychical fact, whereas its object does not exist. So a cognition is certain if and only if it is a cognition of itself. If a cognition is its own object, it is self-contradictory to say that perhaps it exists but its object does not ; if a cognition is not its own object, then (by miracle at any rate) it *may* exist without its object, for no absolute entity logically involves the existence of another.

This once granted, the Aristotelian doctrines as to our natural knowledge can be knocked down like ninepins. A substance is not *identical* either with a sense-content or with an intellectual concept ;

so there can be no certain knowledge of substance. Moreover, even an inductive probability that substance exists would have to rest on past observations of constant conjunction ; but substance is in principle unobservable, for it is not *identical* with the content of *any* intuitive cognition. The Aristotelian view that we can know efficient causes collapses ; for causal laws, not being analytic, cannot have the guaranteed certainty that belongs only to results of the law of non-contradiction. The formal cause can never be certainly known, because the form, say, of stoniness existing in a stone is not *precisely* the same as the content of my concept *stone*. Nicolaus regards the material cause—the permanent subject of change—as a mere manner of speaking ; *we speak* of ' something ' that is first A and then B, but nothing obliges us to do so, and there need not be a ' something ' that *actually* persists. As to the final cause, he easily shows how vague and arbitrary was the way the scholastics used to talk about degrees of ' nobility '. Substance is ' nobler ' than accident ; yet intellect, the power of producing a mere state or accident of the soul, is ' nobler ' than the power of generating a new substance. Moreover, on his own special grounds Nicolaus rejects the distinction of the real and the apparent good or end. Just as cognitions are self-consciously and certainly true, so appetitions are self-consciously and certainly desires for the good ; and no ' ultimate ' end, not psychically given, can be certain. Finally, *all* the arguments for God's existence, resting as they do on such uncertain foundations, must fail to give us any certainty that he exists.

Nicolaus is not here concerned to reject, or transform, or even analyse, such concepts as *cause* and *substance ;* he is merely doubting on principle whatever is not irrefragably certain. He is not a phenomenalist ; for while we cannot be sure of the external existence of a substance or one of the four Aristotelian causes, we likewise cannot be sure that there is no such thing outside us. Nor is he a sceptic ; we are not meant to remain in suspense of judgment. Although only the law of non-contradiction, and our immediate experience, are *certain*, we can none the less construct in detail a *probable* account of the world. Similar as Nicolaus is to Descartes, we notice a great difference in the devices they use to get beyond the first irrefragable certainties. Descartes appeals to a God whose existence and veracity are demonstrable ; Nicolaus has rejected such an appeal in advance. Instead, he asserts as ' probable ' certain ' Principles of the Good ', according to which the world is a supremely perfect whole of perfectly adapted parts. But in view of his own criticisms of the idea of ' nobility ', it is hardly consistent for him to regard these Principles as even significant, let alone ' probable ' ; and, as Dr. Weinberg says (p. 138), how *can* there be adaptations, on Nicolaus's view that no relations are real outside the mind ?

However this may be, Nicolaus goes on to infer that our cognitions can scarcely one and all fall short of their proper end, which

is truth; and if there is to be any reliable sign of truth, it must surely be nothing other than clearness and subjective evidence. This justifies a reasonable confidence in the testimony of the senses, especially when corroborated by other such testimony; and also in logical conclusions more complex than mere cases of the laws of non-contradiction. Dr. Weinberg calls this last result 'self-stultifying' (p. 192); but I think its place in the system is inevitable. There is a formally similar circle when Descartes proves the existence of God by reasonings whose validity may be doubted unless the divine veracity has *already* been established.

Nicolaus infers also some startling metaphysical results. The perfect, for Nicolaus as for all scholastic thought, is the eternal; so the world, being 'probably' perfect, is 'probably' composed only of eternal entities. These eternal entities are as follows: souls; atoms and void; mental acts; *sensibilia*; *imaginabilia*; and finally universals, including *intelligibilia*. The only possible changes are various sorts of motion or rearrangement of these eternal entities. Atoms move locally in the void. Mental acts move 'spiritually' from one mind to another; if I cease to think or feel or desire something, that very thought, feeling, or desire must forthwith come to be in some other mind; and a desire *e.g.*, to go to Notre Dame must be fulfilled at some point in its endless journeying from mind to mind. *Sensibilia* exist eternally, whether sensed by me or not; so do *imaginabilia*, whether I go on imagining them or not. (As Dr. Weinberg points out, it would in fact be enough for the *constituents* of *imaginabilia* to be eternal, while they themselves were mere temporary collocations of eternal elements; p. 203). Finally, universals have eternal, independent existence. They are of two kinds; qualities of things, and *intelligibilia*. Qualities of things, *e.g.*, pure whiteness, are numerically identical and individual in all their instances; they neither come to be nor cease to be—they only come to be *here* after being *there*. Nicolaus himself refers to Plato; one is also reminded of Russell's individual qualities in *An Inquiry into Meaning and Truth*. In a similar way, *intelligibilia*, mental contents, like *fatherhood* or *substance*, are numerically identical in all minds that think of them; and although such contents cannot exist in external objects, their reality is in no way a product of the mind; it is independent and eternal.

The configuration or arrangement of the eternal objects involves no real relation in addition to them; for such an entity, not being eternal, is excluded by the Principles of the Good. Similarly, there is no distinct reality called motion or process; there are no 'flowing forms' in addition to forms that remain in objects. The eternal objects form configurations by what Nicolaus calls 'indistinction', not by means of relations; and these configurations are all that 'happens'; the law of non-contradiction is enough to ensure that different configurations of the same objects are non-simultaneous and are thus successive. (*Cf. Tractatus Logico-Philosophicus*,

2.027-2.03. Wittgenstein's ' objects ' form configurations as links form a chain, without any *further* connexion ; they *exist*, their configurations *happen*). And when the great clock of the stars and planets once more shows the same time, the present configuration will recur—and this an infinite number of times.

I pass over what Nicolaus says about atoms, void, and the continuum. He follows the Greek atomists closely in his account of generation and corruption, growth, nourishment, and alteration ; and in postulating minimal distances and time-intervals. He has little of his own to say here ; and is, as Dr. Weinberg points out (p. 158), extraordinarily incoherent.

Nicolaus makes a very interesting contribution to psychology. He wholly rejects the idea that clearness or intensity is an attribute of mental *acts* ; if I see a clear white and you an obscure white, the difference falls wholly on the side of the *sensibilia ;* it is *not* that you and I see the same white with different degrees of clarity or intensity. (This view was probably devised (*cf.* p. 194) to answer Egidius's objections to the uniform clarity of all certitudes). But when a higher degree of a perfection is present, all lower degrees are compresent with it ; this explains why, as the light fails, white seems gradually to turn to black ; really, the greys and the black accompanied the white all along. I suppose Nicolaus's idea of the divine knowledge—that God conceives all things in conceiving their ' eminent ' being—is in line with this ; the lower perfections of creatures would be literally compresent with the ' eminent ' perfections of God, which are the proper objects of God's concepts. (Dr. Weinberg curiously finds this view of God's knowledge inconsistent with Nicolaus's other views—p. 206).

Similarly, the difference between sensation and intellect is a difference between *objects* of awareness ; and it cannot ever be literally the same thing that we understand and sense. The sensible whiteness is more perfect than the intelligible whiteness, because it is clearer. And no concept or sensation gives us comprehensive knowledge of external reality ; we must be content with piecing many cognitions together to approximate to the existential reality, which in itself may well be one and simple. We know whether different concepts are manifestative of one or more realities by their accordance or discordance ; *true* and *good* may answer to the same existential reality, *man* and *ass* cannot.

(At this point Nicolaus sketches, purely as a speculation, a monistic theory. God and souls are the only existential realities ; concepts that we *take* to refer to different external things all refer either directly to God, or to the concepts other souls form of God, or to concepts that yet other souls form of *these* concepts, or . . . But he does not take this very seriously.)

I do not in the least understand Nicolaus's positive theory of causation. His *concept* of causation is a commonplace idea of efficient causality ; for his objection to Aristotle was purely

epistemological—that causes cannot be *known with certainty*. He thinks that the Principles of the Good give us 'probable' information about causes; and that they imply that a single cause can produce, and must produce, a single effect proportioned to it. This inference is supported (p. 216) by arguments about 'nobility', of the sort that Nicolaus ridicules in others. What remains completely obscure to me is how efficient causality can have a place in a world of eternal objects that only change their configurations; for it is not the configurations but the *objects* that are said to be produced, 'in all instants of time' (p. 218). I do not see what this production of one eternal object by another explains, or what it can mean. In the light of this theory Nicolaus puts forward the 'probable' view that God does not produce, even in part, *any* finite effect; he could produce only one effect, and that an infinite one—if indeed 'infinite God' means anything at all (p. 219). It really is not so very surprising that Nicolaus's work was judged heretical; we need not assume some obscure malice against him, as Dr. Weinberg does.

Dr. Weinberg, in fact, displays a certain *animus*, which I shall allow to speak for itself. On p. 3 he declares that the importance of Nicolaus consists in his 'successful refutation', 'annihilating criticism', of the repeatedly resurrected 'error' ('only thus can' it 'be described') of Aristotelianism. He finds the condemnation of Nicolaus difficult to understand (p. 5); surely his judges must have made a mistake—if indeed they were not guilty (p. 115) of 'a deliberate fabrication due to causes which we cannot now understand'. On p. 162 Dr. Weinberg says he believes Nicolaus's queer idea of velocity is 'an anticipation of the modern doctrine of inertia'. (Newton's, or Einstein's?) On p. 124 Nicolaus is commended for holding contradictory views at the same time—a procedure to which there is 'no fundamental objection', and that has 'the best authorities' of modern science to support it. Needless to say, Nicolaus is praised for anticipating Hume's critique of causation and substance (p. 228); and for putting forth a 'doctrine' that 'almost every generation of philosophers has to learn all over again', and that our own has learnt from—Professor Ayer (p. 35). And Dr. Weinberg ends his book by expressing a pious hope that Nicolaus 'damaged the fabric of scholasticism', and 'perhaps was indirectly responsible', by doing down Aristotle, for 'the rise of experimental science' (pp. 229-230).—It must however be emphasised that in the greater part of the volume Dr. Weinberg sticks to exposition, which is decidedly his forte; one is only a little annoyed at his trying to make out, in occasional remarks, that Nicolaus was a champion of Truth against Aristotle's errors and the ignorance and malignancy of his own contemporaries. As for the defence of self-contradiction by an appeal to 'the best authorities', I am sure Nicolaus would say: *non tali auxilio nec defensoribus istis*.

In explanatory development of Nicolaus's thought Dr. Weinberg shows great skill in avoiding anachronisms; he seems to have

thought himself under Nicolaus's skin. I have noticed only one important instance to the contrary. Such expressions as ' at time *t* ' (pp. 168, 172) are out of place in expounding scholastic views of time and motion. For a scholastic, ' Socrates is sitting ' is a complete proposition, *enuntiabile*, which is sometimes true, sometimes false ; *not* an incomplete expression requiring a further phrase like ' at time *t* ' to make it into an assertion. (The following passages of Aquinas are of interest because they refer to then current disputes about time and temporal propositions, and thus give a background for what Nicolaus says : *De Veritate*, q. 1, a. 5, 6 ; q. 2, a. 13 ; *Summa Theologica*, I^{a} q. 16 a. 8).

I have one last general criticism. It is a pity Dr. Weinberg should keep repeating (pp. 137-139, 154-156, 168, 174) the familiar legend that (i) the typical scholastic view depreciated relative terms, and regarded them either as reducible to absolute predicates, or else as merely expressing *our* ways of conceiving things; (ii) this view has been refuted by modern logic. (i) Of course *many* scholastics held this view—Nicolaus for one. But on the other side Aquinas wholly rejected this way of thinking. (The legend is so widespread that I think it well to give references to key passages in the *Summa Theologica* : I^{a} q. 13, a. 7 ; q. 28, a. 1, 3 ; q. 40, a. 2 ; q. 42, a 1, ad 4 um ; a. 3, ad 2 um ; III^{a} q. 35, a. 5).

(ii) Modern symbolic logic provides technical devices for reducing relative terms to predicates ; *cf.*, Quine's *Mathematical Logic*. I may perhaps sketch one method here. Given Abraham and Isaac, we may form groups whose members are (A) Abraham and the group Abraham-and-Isaac ; (B) Isaac and the group Abraham-and-Isaac. (A) and (B) are different groups and can have different properties. If (A) has a certain property F—which entails that (B) has a property S—then Abraham and Isaac are father and son. (Of course F and S are *not* the properties of being a father and being a son.) And an upholder of the ' scholastic ' view might add that (A) and (B) have no existence apart from our ways of thinking about Abraham and Isaac, so that relative terms answer only to our modes of conception. If we have what Russell calls ' a robust sense of reality ', we shall *feel* that there is something wrong here ; but there is no obvious *formal* objection.

There are certain minor corrections that should certainly be made if a second edition of this work is called for (and the merits of the book are great enough to arouse the hope that one *will* be called for). The parenthesis on p. 186 (first paragraph) is one which I cannot construe. There are certain oddities of vocabulary : ' propositions mental ' (p. 23), ' fragible ' (p. 176), ' supposit ' as a verb (p. 95), ' conversively ' (p. 186). (The last, surely, ought simply to be ' conversely ' ?) The common scholastic locution *quia est*, which like *quod est* simply means ' that it exists ', seems to be what is represented on p. 142 by ' the concept *since it is* ' (author's italics). And I think it is a mistake to consistently render Nicolaus's

subiectivus by 'subjective' and *obiectivus* by 'objective' (pp. 197 *et seq.*). These adjectives have almost exactly opposite meanings to their English derivatives as they are ordinarily used; it is irritating to have to remember that whenever Dr. Weinberg speaks of 'subjective reality' he means objective reality and *vice versa*. It is not as though Dr. Weinberg could not have devised suitable English renderings; he himself suggests (p. 197) 'existential' for *subiectivus* and 'representative' for *obiectivus*.

Translations from the Latin almost always read clearly; but there are several examples of bad mistranslation, where the translator blunders on in amateurish literalness, apparently not recognising the commonest scholastic expressions or even the syntax of the Latin. Without verifying the original, one can tell that something is very wrong with the translation of Nicolaus on pp. 63-64; and of Henry of Harclay, on p. 88. And comparison with the original of the long passage (pp. 165-166) translated from 'Scotus' *De rerum principio* reveals a series of gross errors and lesser misunderstandings. It is unfortunately obvious that Dr. Weinberg's scholarship is by no means on a level with his skill as an expositor of Nicolaus; he turns 'Scotus's' interesting, and pretty clear, argument into an ill-punctuated and barely grammatical string of obscurities. I will give two of the worst examples. To translate *forte instabis* as 'you will object strongly' is a schoolboy howler. We get another such howler in the rendering of *idem sunt re quod fit et quod factum est* as: 'they are the same in the thing which is being made and has been made'. Besides making *re* the antecedent of *quod*, Dr Weinberg betrays here (and in other places) his misunderstanding of the very common scholastic phrase *idem re*. The correct translation is: 'what is coming to be and what has come to be are in actuality the same.'

But scholarship is not everything; and to have made the thought of an almost forgotten philosopher come really alive for modern readers is a considerable achievement. Men far more scholarly than Dr. Weinberg have utterly failed to do this for authors far better known than Nicolaus. Nicolaus is not really an anticipator of 'modern critical views', in spite of what the publishers say on the dust cover; but those of us who enjoy reading speculative philosophy will find him immensely exciting; and we owe Dr. Weinberg a great debt for exhibiting to us so well Nicolaus's uncommon virtuosity in sustained argument. I am sure many readers will find, as I did, that they cannot lay the book down until they have finished it.

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VII.—NEW BOOKS

Les Dialogues de Platon. Structure et Méthode Dialectique. Par VICTOR GOLDSCHMIDT. Paris: Presses Universitaires de France, 1947. Pp. xii, 376. 400 frs.

Le Paradigme dans la Dialectique Platonicienne. Par VICTOR GOLDSCHMIDT. Paris: Presses Universitaires de France, 1947. Pp. 140. 150 frs.

DR. GOLDSCHMIDT, who has previously published a work on Plato's *Cratylus*, now gives us two books on Plato's methods and theories of method. He displays a great familiarity with Plato's texts, and interprets Plato almost solely by means of Plato. Except for a few Aristotelian conceptions, unacknowledged as such, he resolutely eschews all comparison of Plato with later thinkers. He eschews also all criticism; and the reader, though he may reasonably infer that Dr. Goldschmidt feels considerable affection for Plato's doctrines, cannot tell at all whether he holds them true.

In the larger work the title, *Les Dialogues de Platon*, is printed in much bigger type than the subtitle, *Structure et Méthode Dialectique*; and this may suggest to some that the book is a general survey of the dialogues, like Shorey's *What Plato Said* or Taylor's *Plato The Man and his Work*. But, while Dr. Goldschmidt goes into most of the accepted works in detail, he confines himself always to their dialectical aspects, that is, to the philosophical methods which they either exemplify or discuss; and he has no discussion of the *Apology* or *Menexenus* or *Laws* or *Timaeus* or *Critias*, no doubt because he holds that the dialectic is not to be found there.

In all the other accepted dialogues, Dr. Goldschmidt finds the dialectic present to a degree that will surprise many of his readers. The view that Plato's dialogues often fail to practise the kind of method they recommend is one for which he has no sympathy. He finds a single elaborate theory of method, described piecemeal in various dialogues, but practised to a considerable extent in all of them (always excepting the five listed above). The four key words of this method are Image—Definition—Essence—Science. These are, he holds, the four temporal parts of an exercise in Platonic method, and at least three of them are always to be discerned, though sometimes only faintly, in a Platonic dialogue.

The first stage, the Image, is, I gather, the first stirring of dialectic in the man who is still living in the sensible world. It consists in the half-blind and blundering attempt to seize the reality of things by images. Dr. Goldschmidt finds it exemplified somewhere near the beginning of every dialogue. He finds it referred to in what the Line says about the mathematicians' use of sensible images, and in what the *Seventh Letter* says about the place of the *idolon* in knowledge.

The Image leads on to the Definition, also referred to in the epistemological part of the *Seventh Letter*, but described best in the *Phaedrus* (265D): 'To see the scattered many as one, and bring them into one Idea, in order that by defining each thing he may make clear whatever it is that he wishes to teach.' The Definition always comes suddenly; for there is always an element of inspiration or revelation in it. On the other hand, it frequently involves reasoning.

'The search is not yet finished for all that. If we now really have definitions in the sense in which the *Phaedrus* used this word, we must expect to see a division arise in the midst of this unifying ⟨definitory⟩

notion. We are now on the level at which love would be defined as a madness. But this is an ambiguous notion, and can justify equally well either blame or praise, according as one " takes the left side or the right ". Which way should we choose ? ' Thus arises an ' alternative ' which must be settled, and the settling of it is the third stage of Platonic method, if I understand Dr. Goldschmidt rightly, the stage he calls ' Essence '. It is the stage indicated in *Letter Seven* by the words *οὐ τὸ ποιόν τι, τὸ δὲ τί*, and described in the Divided Line as the upward path of dialectic leading to an unconditioned principle. It usually appears as a long digression, and the author gives it the technical name of ' the essential detour '. Its success depends on our choosing the righthand alternative, which is the good, and avoiding the left or bad one. In the ' aporetic dialogues ' the wrong choice is made, and consequently Essence never appears, except as a suggestion briefly put forward by Socrates and unrecognised or rejected by his ' pupil '.

In the ' completed dialogues ', on the other hand, Essence is more or less attained, and hence it is possible to introduce the fourth and last stage, Science, which is indicated in *Letter Seven* by the word *episteme*, and described in the *Republic* as the downward path of dialectic from the unconditioned principle. Once the previous stage has been successfully accomplished, this last stage is easy and sure and certain.

In the *Republic* Image is the first account of the ideal city in Books II-III ; Definition is the definition of justice and the other virtues in Book IV ; Essence is the further account of the ideal city in Books V-VII ; and Science is the last three books. In the *Phaedo* the Image runs to the end of the reminiscence-argument for immortality ; Definition is the argument from the incompositeness of the soul ; Essence runs from the objections of Simmias and Cebes to the middle of the last proof of immortality (105B5) ; and Science runs thence. Whether Science includes the myth I do not know. In this dialogue the long third section, Essence, can again be divided into the regular fourpart scheme. In the *Banquet* Image runs to the end of Socrates' examination of Agathon ; Definition runs thence to the end of Diotima's doctrine that love is of a middle nature, neither rich nor poor, neither wise nor ignorant (204C7) ; Essence runs from her doctrine that the object of love is good for oneself for ever to the end of her speech as reported by Socrates ; and Science is the speech of Alcibiades on the character of Socrates.

Dr. Goldschmidt develops this fourpart scheme by means of many more methodological concepts, some drawn from Plato, some from Aristotle, and some I think his own invention. Thus he takes Plato's notion of hypothesis and makes it, if I understand him rightly, a transitional stage between Image and Definition. ' Let us, in accord with the *Phaedrus*, mean by a ' definition ' an hypothesis that is fertile, that gives us a sufficient view of the whole to delimit the object.' (I think he has *Phaedrus* 265D in mind, but I do not agree that this or any part of the *Phaedrus* puts forward such a proposal.) He takes the two tests of hypotheses in *Phaedo* 100-101, the test by the consequences and the test by a higher hypothesis, and puts them into the stage of Essence. For example, in the Essence part of the *Phaedo*, it seems that 100-103 is test by consequences and 104-105 is test by higher hypothesis. In carrying out this idea Dr. Goldschmidt seems to degrade the *Phaedo's* notion of ' higher ' till it means merely ' better ' and all distinctively logical content is lost.

Among the concepts that Dr. Goldschmidt appears to have invented for himself is that of the Exigences Essentielles or Essential Demands. These

are propositions such as 'Courage is a very noble thing' (*Laches* 192C), expressing an obscure foreknowledge of Essence. 'Let us call these propositions "Essential Demands". They deal with Value, with Essence, prior to all definition. They do not state a quality, though they may seem to. To say that Courage is good (though one does not yet know what Courage is) is not to collide with the methodological criticism that one must not say "Meno is handsome, rich, noble" if one does not in the least know who Meno is. To say, Courage is good, means: Courage is a Value. To recognise goodness in Courage is to yield to the appeal that Essence makes to us, to worship a god as yet unknown, but whom we know to be a god, whatever his nature may be. It is not we who confer upon him such and such an attribute, who "postulate" such and such a quality for him. It is he who "wishes" to be thus called; and we merely obey his *Demand*.' These Demands appear in the criticism of the hypothesis. (They are, in my language, such ethical commonplaces as Socrates requires as premisses for the refutation of the theses put forward by his companions.)

Among other terms that frequently recur in a technical methodological sense are: aveu, critère, prétention, obstacle, catégorie infamante, apposition, directeur d'études, précepte d'unification, tentation, glissement, détermination, et alia, oppositum, et idem non.

This elaborate network of methodological conceptions, which Dr. Goldschmidt patiently and craftily casts at each of the Platonic dialogues in turn, does not in my judgment catch and hold any one of those lovely slippery creatures. Each and all of them wriggle out of his contraption and go their highly individual unclassifiable ways. He might have tamed them more if he had tried to tame them less, merely pointing to an occasional feature here and there instead of insisting on finding the same scheme in all of them. Some of his concepts would have had to be dropped altogether; but some might then have been useful. Be that as it may, we can be grateful to him for calling our attention once again, and in a novel way, to the question what method the dialogues actually display, and whether it is the same as any of the methods they talk about.

The shorter book, *Le Paradigme dans la Dialectique Platonicienne*, is an essay on the account of paradigm in the *Statesman* 277-9 and its significance for the rest of the dialogues. Dr. Goldschmidt links this passage with a great many of the illustrations and similes and analogies in the various dialogues. Many of the technical terms of the larger book appear here too; and there are some peculiar to this book, including 'Forme paradigmatique', 'Forme précieuse', and 'Forme-élément', which I have not understood. Dr. Goldschmidt concludes that Paradigm occupies a very important place in Plato's dialectic. Sometimes he says it is practically identical with dialectic. At the least it is co-ordinate with Reminiscence as a way of discovering Essence.

This estimate of the importance of Paradigm naturally involves holding that Plato's dialectic was less opposed to the use of the senses than we commonly think. Dr. Goldschmidt writes: 'For Plato the visible world "really exists", and the dialectician, far from being "the touching and slightly ridiculous figure" of the *Theaetetus*, has a strong sense of reality and a taste for *Observation*'. The evidence offered is a score of passages in which Socrates is made to say 'Haven't you observed that . . .' or something of the sort. This would be good evidence for Dr. Goldschmidt's view only if everything that could truly be said about the Platonic Socrates were part of Plato's theory of dialectic. An account which we can give of what Socrates does in the dialogues is not the same as an account given by

Socrates in the dialogues of what the dialectician does. It seems much more likely that Plato never regarded the remarks cited by Dr. Goldschmidt as implying that the dialectician has a taste for observation.

In pursuit of the same view Dr. Goldschmidt says that Plato's method of Paradigm includes a process of *verification*; and he tries in vain to persuade us that Plato sometimes meant verification by the words '*ἐπισκοπεῖν*', '*βασανίζειν*', and '*ἐξετάζειν*'. It seems clear that this error results from his associating the *Statesman's* account of Paradigm with the city-man analogy in the *Republic*. The Socrates of the *Republic*, after having discovered justice in the example of the city, undertakes to confirm that this is true of the individual also by various arguments and inquiries. This is represented as a process of discovery, which can be separated into two stages, (1) getting the idea, and (2) testing it. When I read the account of Paradigm in the *Statesman*, however, I seem to find quite definitely and unambiguously that the Stranger does not represent it as a process of discovery at all, but of pedagogy pure and simple. I am nonplussed by Dr. Goldschmidt's reading of the text. And against his view on page 98 that Paradigm is a process of research I quote his view on page 110 that the aim of Paradigm is 'to communicate a doctrine'.

(There is one other very odd reading of the text in this book: Dr. Goldschmidt takes the sentence '*Σὺ δέ γε.., οἶμαι, οὐκ εἰωθας δὶ εἰκόνων λέγειν*' (*Rp.* 487E) to be not ironical but literal. He is therefore puzzled to understand how Adimantus could make such a blatantly false statement, and is led to suppose that 'for Plato the word "image" has two different, not to say inconsistent, meanings'.)

In sum, Dr. Goldschmidt's interpretations of the text are not always so rigorous, nor his logical conceptions so much criticised, as readers of MIND may require; but his books are valuable to philosophical students of Plato for their rich acquaintance with the text and their copious flow of methodological ideas about it.

Each book contains lists of passages, Greek words, topics, and authors, discussed. The smaller book also contains a list of books relevant to its subject.

RICHARD ROBINSON

The Bhagavad-Gītā, With an Introductory Essay, Sanskrit Text, English Translation and Notes. By S. RADHAKRISHNAN. London: George Allen & Unwin Ltd. Pp. 388.

THE design of Professor Radhakrishnan's new work, which is adequately particularised in its title as above, is explicit in the Preface. Starting with a reference to present world conditions and the opportuneness of the *Gītā* as a text of religious reconciliation, the author observes that:

> 'History poses our problems, and, if we restate old principles in new ways, it is not because we will do so but because we must. Such a restatement of the truths of eternity in the accents of our time is the only way in which a great scripture can be of living value to mankind.'

The avowed principle, not absolutely repudiated in the study of old literary and philosophic texts, has an admitted application to religious writings, which, ruling during long periods and with superior authority over larger regions of human life, must be adaptable to mentalities of varying proclivity. The *Bhagavad-gītā*, a text which during perhaps two millennia has been invested with an authority analogous rather to that of the Gospels in Christianity than that of Plato in philosophy, is unquestionably a 'scripture'. That in fact it deals with momentous matters of perennial

interest is obvious: and it has constantly had to sustain the anxious scrutiny and adaptative interpretations of new epochs. Modern scholarship, largely absorbed in the complementary task of relating the doctrines and composition of the *Gītā* to its antecedents, has never been insensitive of the literary qualities to which Professor Radhakrishnan goes on to refer or of the significance of its themes in the history of Indian thought. The situation is therefore favourable to an interpretation and valuation of it in the light of universal religion.

The Introductory Essay, marked by astonishing energy of thought and corresponding ease and effectiveness of expression, deals succinctly with the importance, criticism, date and philosophic-religious antecedents. The next section states the facts and views relating to the historical personality of Krishna and proceeds to discuss the notion of his divinity and that of his *avatāra*, 'incarnation', which, perhaps more epigrammatically than fundamentally, turns out to be 'not so much the conversion of God-head into flesh as the taking up of manhood into God'. The remaining bulk of the Essay is devoted to the chief doctrinal ideas of the text, 'The Status of the World and the Concept of Māyā', 'The Individual Self', *Yoga-śāstra* (discipline of 'binding one's psychic powers, balancing and enhancing them'), *Jñāna* or Saving Wisdom', 'The Way of Knowledge: *Jñāna-mārga*', 'The Way of Devotion: *Bhakti-mārga*', 'The Way of Action: *Karma-mārga*', 'The Goal'. In case any reader should suspect that the eloquent expositions of these topics owe more than a modernity of expression to Professor Radhakrishnan's very wide familiarity with philosophic and mystical literature, he would misapprehend the range of what India would tolerate in an interpretation of the *Gītā* and the evidence of the telling quotations in the notes to the Essay and the text.

To readers of MIND it may occur that some of the topics hardly come within their special sphere of interest. The matter, for instance, of incarnation, sometimes diluted in India, as perhaps elsewhere, to the extent of regarding every man as in some sense a vessel of divinity, is hardly explorable without reference to the actual history of facts and sentiments. Professor Radhakrishnan's own view would to a certain extent countenance the exclusion or reservation: he everywhere avows the conviction expressed in the statements that 'ignorance is not intellectual error. It is spiritual blindness' and 'wisdom is direct experience which occurs as soon as obstacles to its realisation are removed'. But this view, nowhere more explicit than in Indian Jainism, has to be supplemented by the likewise original Indian understanding expressed in the proposition that 'Wisdom, pure and translucent, is different from scientific knowledge, though it is not discontinuous from it'. The final insight was the culmination of a long effort of intellectual clarification: its unspeakableness, or unthinkableness, is not that of the muddle of common speech, for which everything is equally denotable by symbols (in India ultimately by a mere negative), but the attainment of a transcendent enlightenment coextensive with the all. It was in the *Bhagavad-gītā* itself that the intellectuality which always, but in co-operation with dogma, characterises the standard expositions of Indian systems, was first invaded by explicit formulations of easier routes such as *bhakti*, the love of a loving God. This *bhakti*, presented in the *Gītā* as a not exclusive, though superior, alternative, is the most prominent of the 'apparently antithetical forms of the religious consciousness', which Professor Radhakrishnan finds thus harmonised in the *Gītā*.

The special ethical doctrine of *karma-yoga*, that is, action deliberate,

poised and void of every self-regarding motive, invites comparison with the Kantian principle of doing that which one would approve as law universal. First broached in an Upaniṣad as a deep subject, action, especially as human or moral action, had passed through a tornado of discussion, largely but not exclusively, relating to its possibility or value for a thinking subject or, in some views, a denial of the latter. In the *Gītā*, where only the main antithesis of action and thought appears, the former is recommended on the ground of its inevitability even for divinity itself, but more fundamentally, and in the spirit of the text, on devotion of it to Krishna himself, who, as indweller of the inmost soul of the agent, is the real doer of all. This is fully expounded by Radhakrishnan, who also elicits a view, not very explicit in the text, of free-will as something provisionally existing in the design, or by the grace, of the divinity.

Duties, virtues and vices, again, appear as matters well understood and no longer calling for analysis and classification; the subject of duty (*dharma*) and duties was, in fact, thrashed out in special treatises; and the whole great *Mahā-bhārata*, wherein the *Gītā* is episodical, belongs, as a *dharma-samhitā*, '*dharme*-compendium', to a period of rehabilitation. A new term, however, *loka-samgraha*, 'care for the people', 'public spirit', receives emphasis as being, on a universal scale, a character of Krishna himself, whose incarnations take place when there is need of a Saviour to uphold the right.

What of metaphysics is detectable in the *Gītā* belongs, as Professor Radhakrishnan shows, to the Sāmkhya system, already somewhat definitely formulated and so commonly followed that in the text the name is employed as a general term for discriminative thought. The concluding cantos deal with a number of psychological and social topics, temperaments, classes, diet, giving, etc., on the lines of the three-*guna* (constituent) theory of the Sāmkhya. Here Professor Radhakrishnan is at pains to show a reasonableness in notions which the reader may be content to regard as ancient.

It may perhaps be judged that the main speculative interest of the *Gītā* lies in what is in fact its main tenet, personality in an author and sustainer of the universe. In a rather mythological form the conception was in India far more than the Upaniṣad doctrine of the impersonal Brahma-ātman and perhaps was never below the horizon. When it re-emerges in force in the *Gītā*, it boldly claims to incorporate its rival. But neither does the text itself support the claim otherwise than by its inspired pronouncement as 'wisdom more secret than all secrets' (p. 375), nor would Professor Radhakrishnan see any point in a dialectical examination of it. 'The question whether the *Gītā* succeeds in reconciling the different tendencies of thought will have to be answered by each reader for himself after he completes the study of the book.' Throughout the brilliant Introductory Essay, in which he elaborates many other matters implied or stated in passages of the poem, and again in many fine quotations illustrating the exegetic and interpretative commentary, his standpoint is that of *γνῶσις* more than that of *ἐπιστήμη*. The above quoted statement concerning the limitations of the intellect allows a more general Reason, as in fact any disquisition must do. But it lays stress on experiences and humanities, among which it may perhaps include the attractiveness of the Krishna of the *Mahā-bhārata* and *Gītā*. The still unexhausted protreptic force of the ancient literary-religious masterpiece may ensure the success of Professor Radhakrishnan's design of making its comprehensive outlook and philosophic implications effective in general religious enlightenment.

F. W. Thomas.

The Freudian Psychology and Veblen's Social Theory. By LOUIS SCHNEIDER. New York, King's Crown Press (London: Geoffrey Cumberlege). Pp. x, 270. 18s.

THE choice of title is not a happy one. It does not give any indication of the problem with which Dr. Schneider is concerned, and it calls to mind those wearisome comparisons in which a bright idea is nursed into a little flame, only to expire into obviousness—or worse. There is more to Dr. Schneider's book than that. He is really engaged in an attempt to clarify one of the main issues of social psychology, namely the relation between that subject and sociology, and he uses the comparison between the theories of Freud and Veblen as a vantage point from which to survey the scene.

Briefly the Freud-Veblen comparison comes to this: both writers stress the non-rationality of human behaviour, both criticise social institutions, but, whereas Freud ignores the independent and socially accepted character of institutions, Veblen gives full weight to this, only to imply that man would be much better without any institutions at all. Both of them, therefore, in Dr. Schneider's words are 'anarchistic'.

It should be noted that Dr. Schneider uses the word 'institution' in a somewhat wider sense than would be acceptable to most sociologists. He includes under that heading all accepted 'norms' and beliefs—such for example, as the belief current in Germany that the Reich was being encircled with hostile intent. This wide usage is justified—though not explicitly—by the fact that Dr. Schneider is stressing all the time the 'normative' aspect of institutions. "In any society", he says, "norms are of the essence of the institutional structure and represent values from the point of view of the agents holding to them." It is a short step to say that publicly accepted values are part of the institutional complex, and for his purpose the step may be taken.

Now for Freud the institutional complex is an unpleasant necessity, required to curb the aggressive libidinous violence of 'natural' man, who stands over against this restraining influence, which he has precipitated, in discontented rebellion. As Jung observes, "the whole edifice of culture becomes a mere surrogate due to the impossibility of incest". The institutions are psychologised, and their quasi-independent quality is ignored. For Veblen, on the other hand, the institutions of capitalism are analysed in all their objectivity. The monetary measure of value, the financial apparatus, the 'leisure class' ethos, with its standards of 'conspicuous waste' and the 'degradation of labour', and the conflicting aims of industrial efficiency and of profit-seeking—all these, according to Veblen, have us in their grip. Their independence is emphasised. For Veblen, it is true, man is 'naturally' co-operative, and 'naturally' endowed with an 'instinct of workmanship', so that he stands hostile to the institutional complex, but in a different way from that envisaged by Freud.

Out of all this emerge three notions: anarchism, the psychological nature of man, and social norms, which, says Dr. Schneider, "have a certain independent status". So far as 'anarchism' is concerned, the Neo-Freudians, by whom Dr. Schneider means such people as Adler, Horney, Linton, Fromm and Kardiner, have toned down the orthodox view. For them the main-spring is the need for security in society, and sex is but a field in which a sense of insecurity may well display itself. Anyway, man has, in some sense or other, a 'social nature'. Veblen's psychological notions are rightly dismissed as inept.

We now come to the real problem. What conceptual picture can we

devise which will show the relation between the psychological characteristics prevailing in a society and the institutions of that society, a picture, moreover, which will do justice to, and account for, the independent status of the institutional complex ?

The ' naturalness ' of institutions is made clear by their very acceptance. It is all nonsense to confront ' feral man ' with social institutions which outrage his ' nature ', and evidence from the study of face-to-face groups in industry bears this out. Somehow the psychologist and the sociologist seem to be studying the same phenomena from two points of view. As Dr. Schneider puts it : " The individual, from the point of view of clinical psychology, is to be looked on as the locus of a more or less complex value system. From the point of view of social science, the individual has to be looked on as to an extent the bearer of values, insofar as social norms have their impact on him."

The whole subject has, of course, been altered since the American anthropologists have invaded the scene with their psychologico-sociological analyses of primitive cultures. These have revealed how hopeless it is to try to get any idea of man's ' nature ' from a study of Western men alone. The tables have been turned. Men are what they are because of the cultures in which they have been brought up. Once, however, a ' basic personality ' has been established, the members of a society, brought up in its mould, sustain the culture. But there is a multiplicity of cultures, and therefore of ' basic personalities '. The interaction is obvious, but whence the variety ? " Given certain types of individuals only certain cultural goals will seem congenial and worth-while ; on the other hand, given certain cultural goals, only certain types of individuals will find them desirable." But what makes for different types of individuals ? That is one question with which we are left, and Dr. Schneider refrains from commenting on those studies of national character which derive salient features of ' basic personality ' from the intimate details of infant training. The other question is this : granted that " Given certain types of individuals, there cannot simply be *any* kinds of goals ? " What gives such goals as *are* established their institutional objectivity ?

At any rate Dr. Schneider claims that three points have been established by the investigations to which he refers. They are : (1) There is some connexion between " the levels of individual and social analysis ", other than mere opposition or identification. (2) " One important type of connexion between the institutional and individual levels is the connexion of interdependence ". (3) The relation between the two levels can be dealt with in terms of ' congruity ' or ' lack of congruity ' together with " associated conceptions of reinforcement or lack of reinforcement ".

With these points in mind Dr. Schneider proceeds to examine two examples of social phenomena which have been considered at the two levels : the ' leisure class ' complex, and Nazi Germany. With regard to the first he exhibits congruity between Veblen's analysis of the ' leisure class ' ethos, and Horney's analysis of the " neurotic personality *of our time* " (Dr. Schneider's italics). The word ' neurotic ' is, perhaps, misleading, but the point is that there is a ' fit ' between the two analyses. Granted uneasy folk, striving to give themselves a sense of security and worth, then unlimited profit seeking and a monetary measure of worthiness are just the thing. Conversely, a profit-making system must be run by people who find gratification in making money. An all-important warning, however, is given. " It is clear . . . that neither the striving for power nor the impulse to manifest superiority nor basic doubts about being worthy

nor any of the other characterological traits or trends reviewed may be looked upon as *creating* the institutional structure." All one is entitled to say is that the congruity is striking, and we must leave it to future social psychologists to find the explanation.

The puzzle is even clearer when we reflect on Fromm's theory of the congruence between the 'anal character' with its frugal orderliness, and the bourgeois ethos of orderly frugality. If we accept the assertion, then what? Is it sensible to ask which came first?

The analysis of 'Modern Germany' brings out other points. Dr. Schneider very properly deprecates the facile analogical use of clinical terms like 'paranoia' of nations, and with this criticism he demolishes a great deal of nonsense. Again, he points out that the mere assertion that the Germans accepted an ethos of ruthlessness because they are cruel 'by nature' gets us nowhere. Fromm's analysis, however, is more serious. According to him, it will be remembered, the economic position of the lower middle class was such that, having been routed out of traditionalism by the hope of those rewards which await the enterprising, they found themselves exposed to an economic blizzard and had no homely sheltering beliefs to fall back on. Therefore they fell for the Nazi ideology, which removed from their shoulders the burden of thinking for themselves, and at the same time pointed out who was to blame, thereby providing a target for their frustration-generated aggressiveness. Ingenious, and illuminating, but still the problem remains: how does the sense of frustration in the breasts of *separate* individuals find expression in an accepted ideology?

As I have indicated, Dr. Schneider raises questions. They are not new ones—they have been canvassed before by Talcott Parsons—but what Dr. Schneider has done is to sharpen the issues in an extraordinarily sensible way. To have done that is by no means nothing. Let us hope that he will help us to answer the questions he has raised.

W. J. H. SPROTT.

Hobhouse Memorial Lectures, 1930-40. By Geoffrey Cumberlege, Oxford University Press, London, 1948. Price 10s. 6d.

THE subjects of these lectures—social equality, the mind-body relationship, common enjoyment, rational and irrational elements in contemporary society, the unity of mankind, thought and existence, materialism in politics, the economics of public education, the downfalls of civilisations, the decline of liberalism—hardly represent adequately even in their obvious variety, the many interests of the great sociologist and philosopher in whose memory they are now published.

Leonard Trelawney Hobhouse uniquely combined in his work much that is most valuable in the British tradition, and notably a realist Liberalism, and an empiricism broad enough to include among the data of philosophy, the methods and findings of the social sciences. More than anyone, except perhaps Bertrand Russell, he may be regarded as the inheritor of the liberal-empiricist mantle of John Stuart Mill.

Empiricism is often represented in our own day as a technical discipline of which the primary function is the elimination from discussion, as non-significant, of all propositions except those of the physical sciences and mathematics, and other propositions of the same "form". Yet if we take an historical view of British Empiricism, we shall find that John Locke, its founder, laid its foundations broadly enough to provide for the "experimental" approach to human society, with its endless variety of problems.

It is in the writings of J. S. Mill that we reap the harvest of Locke's sowing. From Locke to Mill, the empiricists explored very fully the problems of the new way of thinking, based on actual experience; with which logical and epistemological task they linked the examination of the slogans of the new society: "liberty" and "happiness". If Mill's work be regarded as a whole, it presents the most significant document of an empiricism in which the quest for knowledge is inseparable from the quest for "liberty"—a value whose realisation in practice is recognised to be as difficult as its definition in theory.

The appropriate fruit of the scientific spirit for which, in its wider application, empiricism is another name, is a mind capable of real development. Mill kept his mind alive, and in touch with the practical demands of the human situation of his own society. Trained in Benthamism, with its exaggerated faith in legal and political, as contrasted with economic, reforms, he was forced by the actual events of his time to move towards a critical socialism. The provision of a philosophy for socialism may thus be said to be the chief task which Mill bequeathed to his successors. It has never been adequately tackled. Hobhouse was one of the very few academic philosophers who saw its importance, and then only late in life. In this respect he reproduced, in a remarkable way, the development of Mill, himself, moving steadily toward the Left in politics, under the influence of the 1914-18 war and its sequel of economic crises.

In this movement to the Left, one of the most formidable philosophical obstacles was presented by the Idealist school of Green, Bradley and Bosanquet. The revolt of Hobhouse against this school received its most famous expression in *The Metaphyscial Theory of the State*, a theory of which he said that the bombing of London was its "tangible outcome". Hobhouse was, however, sufficiently tolerant of what was personally uncongenial to him to try to do justice to the idealist ethic, and in his theory of the good as harmonious development in society, he was clearly much indebted to T. H. Green.

In this review I have so far attempted to give very briefly some impression of the work of Hobhouse and its historical background, rather than to summarise or comment in detail on the lectures. These speak very well for themselves, as the names of the distinguished lecturers will indicate—Hobson, Myers, Hammond, Mannheim, Ginsberg, Dawes Hicks, Stocks, Tawney, Toynbee, Laski, (some of whom, alas, have themselves since finished for all time their contributions to social philosophy).

There is hardly a page in this volume which is not brilliant and stimulating. Here I can only mention a few of the remarks which I have found most provocative, and make one or two of the most obvious criticisms.

Hobson re-states the problem of social equality with a refreshing concreteness, but recent data on the distribution of intelligence might moderate even his modest optimism. Hammond makes an eloquent plea for greater public provision of aesthetic education and enjoyment, pointing out that "the growth of common enjoyment" made possible by modern techniques, increases social responsibility in aesthetic matters.

Mannheim's lecture on *Rational and Irrational Elements in Contemporary Society* I found the most challenging and disturbing, especially when one looks at the date, 1934, and realises how little improvement has been achieved, and at what a cost. Incidentally Mannheim provides some good examples of sociological prediction, *e.g.*, "The pilot who only a minute ago seemed to be flying quietly above us hurls a hurricane of bombs and in the twinkle of an eye lays waste everything and annihilates everybody

underneath him " (p. 48. My copy is not paged continuously throughout. Continuous paging would improve this volume, whose contents are also surely important enough to deserve indexing.)

Ginsberg distinguishes five important senses of " the unity of mankind " and demonstrates, incidentally, that the discussion of these need be neither abstract nor emotive : (1) uniformity or similarity of type or character ; (2) interdependence and interconnexion between various civilisations or groupings ; (3) the process of unification ; (4) the subjection of social processes to laws ; (5) a fundamental unity of purpose. (He is not committed to the assertion of these as facts.)

In *The Downfalls of Civilisations*, Toynbee has stated concisely his now familiar thesis of the degeneration of creative minorities into merely dominant ones, alienating themselves from proletariates which then secede from them. His lecture concludes with the usual question (now, I suspect, become merely rhetorical) : " Is the Church the heir of the civilisations ? "

Perhaps the main interest of Tawney's *Thoughts on Public Education* is the extent to which, here again, his suggestions anticipate the actual policy of the present government. (This may well be a case of cause and effect, whereas Mannheim could hardly be blamed for the coming air-raids !)

The two lectures on abstract philosophical topics suffer most from condensation, and I shall say little more about them. I think Myers' purpose is to *identify*, in some sense, " life " and " mind ", but the sense remains obscure to me (as perhaps it must).

The general realism of Hobhouse's *Theory of Knowledge* is treated sympathetically by Dawes Hicks, but he is severely critical of its details, *e.g.*, of his account of the role of " sensations " in knowledge. According to Dawes Hicks there are " no such thing as sensations " (p. 17).

Two of the most interesting lectures remain. They are Stocks on *Materialism in Politics*, and Laski on the *Decline of Liberalism*. It would have been better if Laski had lectured on Marxism and Stocks on Liberalism ; for each has too clearly made up his mind in advance of the discussion he here presents, and the verdict is of course unfavourable. Laski shows more knowledge of Liberalism than does Stocks of Historical Materialism. He summarises his main criticism in a quotation from Hobhouse : " Liberty without equality is a name of noble sound and squalid result " ; but Laski tends to underestimate the justice of the liberal fear that equality may, in actual society, be purchased at too high a price in liberty itself.

Stocks unfortunately shows little knowledge of Marxist literature. He thinks that the Marxist view of history is a " fatalist " view, and that it minimises the agency of man. He had glanced through *A Handbook of Marxism* but had not noticed the emphatic declaration : " Man makes his own history " ; nor the equally famous : " Hitherto philosophers have *explained* the world : our task is to *change* it."

I have noticed two misprints : " confined " should be " confirmed " in l. 16, p. 21, and " scourage " should be " scourge " in the penultimate line of the same page of Stocks's lecture.

D. J. McCracken.

Malebranche. Correspondence avec J-J Dortous de Mairan, précédée d'une introduction sur Malebranche et le Spinozisme par Joseph Moreau. Librairie philosophique. J. Vrin. 1947.

The scholarly little work is an edition, the first since 1845, of a correspondence conducted in 1713-14 between Malebranche and an intelligent gentle-

man of philosophical interests on the subject of Spinozism and its relation to Malebranche's own system. The correspondence begins with a request from Mairan that Malebranche shall refute Spinoza, which Mairan himself is unable to do, although anxious to reject his conclusions. Malebranche is scarcely successful in producing the required refutation, but he does succeed in the course of the argument in showing that his " intelligible extension " is not to be confused with Spinoza's attribute of extension and that the crucial difference between himself and Spinoza lies in the latter's rejection of the notion of creation, which leads to a different interpretation of the term, " substance ". Apart from making clear these differences, the letters have considerable historical interest as showing the impact of Spinoza's method on an intelligent mind with some mathematical training. Mairan insists again and again on the rigour of the Spinozistic demonstration, whereas to the modern mind nothing could be more obvious than their looseness and inadequacy judged by standards of mathematical rigour. Malebranche also seems to have been under the same impression, as is shown by the passage from the *Meditations* quoted on p. 117 and also by the fact that while he professes to find Spinoza's demonstrations faulty (p. 103) he is very slow, in spite of continued pressure, to point out the precise fault. Of Spinoza's contemporaries only Leibniz, a practising mathematician of genius, seems not to have been over-impressed by the external form of the *Ethics*.

In the " Introduction ", which occupies the greater part of the volume, M. Moreau elaborates the argument of the letters and points out, often with great acuteness, very interesting connexions and differences between Malebranche on the one hand and, on the other, Spinoza, Descartes, Leibniz, Kant and Plato. He has no difficulty in showing that Malebranche's position does not lead necessarily to Spinozism but rather anticipates in many features the Kantian idealism. In theory of knowledge Malebranche differs from Spinoza in his frank representationalism (M. Moreau is particularly clear in pointing out in the note on p. 19 the non-representationalist character of Spinoza's theory); in ontology in having a different conception of substance, which allows creation to be a meaningful notion.

M. Moreau seems to be mainly interested in establishing these historical connexions and to care less for the possible philosophical truth of either system. This leads to some surprising omissions. For instance, there is a long discussion of the difficulties of Cartesian physics occasioned by the definition of matter as extension (pp. 52 ff.) and M. Moreau explains at length how Spinoza and Malebranche attempt in their different ways to overcome these difficulties by introducing motion in a non-miraculous way. But there is no suggestion that, even so, the difficulties might not be fully met and that some principle of differentiation other than motion is necessary to account for the diversity of the actual.

In some respects I find myself in disagreement with M. Moreau's philosophical judgment. I do not think that he allows enough weight to the arguments of Spinoza and Mairan as compared with those of Malebranche, and I am not convinced by his defence of the ontological argument in a certain form. It seems to me that the criticism of Kant, which as he points out, is partly anticipated by Malebranche, is valid against it in all possible forms. But these mistakes, if they are such, by no means detract from the value of the work as a historical study.

MARTHA KNEALE.

The Mathematical Analysis of Logic: Being an Essay towards a Calculus of Deductive Reasoning. By GEORGE BOOLE. Oxford: Basil Blackwell, 1948. Pp. 82. 7s. 6d.

ALL logicians will be grateful to the present publishers for this reprint of Boole's pamphlet. One of the most important single contributions to modern logic, it was originally published at Cambridge in 1847 by Macmillan, Barclay, and Macmillan, and its centenary was marked in MIND by a very interesting and informative article by Mr. William Kneale in the issue for April, 1948. Copies of the original edition have long been scarce, and so far as I know this is the first reprint. Professor Alonzo Church's *Bibliography of Symbolic Logic* lists the work as having been reprinted in Vol. I of *George Boole's Collected Logical Works*, edited by P. E. B. Jourdain, but as Mr. Kneale points out in his article, this volume was never published. The essentials of Boole's great contribution to logic are now so well known that a description of the contents of the book is hardly necessary; but it should, perhaps, be mentioned that the work contains most of Boole's original ideas, which were later expanded and applied to the theory of probability in the larger and better known *Laws of Thought*. And it may, perhaps, also be mentioned, for the benefit of those philosophers who are either unwilling or unable to make the small mental effort necessary for understanding the main body of the work, that the Introduction (beautifully written) contains a number of interesting general remarks which they could read with profit. (Although some of these remarks, it must be confessed, are rather 'psychologising'.) It is, I think, fitting to conclude this very brief notice by quoting a few words written by De Morgan shortly after Boole's death. 'That the symbolic processes of algebra, invented as tools of numerical calculation, should be competent to express every act of thought, and to furnish the grammar and dictionary of an all-containing system of logic, would not have been believed until it was proved. When Hobbes, in the time of the Commonwealth, published his 'Computation or Logique', he had a remote glimpse of some of the points which are placed in the light of day by Mr. Boole. The unity of the forms of thought in all the applications of reason, however remotely separated, will one day be matter of notoriety and common wonder: and Boole's name will be remembered in connexion with one of the most important steps towards the attainment of this knowledge.' (*A Budget of Paradoxes*, p. 301.)

I have noticed two misprints: on page 31, 'fEstIno' should be 'fEstInO', and 'fEsapO' should be 'fEsApO'.

C. LEWY.

In Defence of Materialism. The Development of the Monist View of History. By G. V. PLEKHANOV. Translated from the Russian with an introduction by Andrew Rothstein. London: Lawrence and Wishart, 1947. Pp. 303. Price 18s.

FROM his "conversion" in 1883 until Lenin became leader of the Bolsheviks Plekhanov was the leading Russian revolutionary Marxist. A large part of his life was spent in exile, and one fortunate result of this was that he not only read widely in European Socialist literature but also, in a long series of expository and polemical writings, endeavoured to show the place of Marxism in its European cultural setting. English students of Marxism

have already been able to read in translation his brief *Materialist Conception of History* and *Role of the Individual in History* and the somewhat longer *Fundamental Problems of Marxism* and *Essays in the History of Materialism*. The present work, now translated into English for the first time, was originally published in St. Petersburg in 1894 under the title *On the Question of the Development of the Monist View of History*. This clumsy title was chosen in order to render the book more acceptable to the censor than it would have been with the title Plekhanov originally had in mind and which this translation now bears.

The book opens with a chapter on French materialism of the eighteenth century, in which Plekhanov brings out the difficulties in maintaining, as these thinkers tried to do, both that men are creatures of their environment and that it is opinion that governs the world. The second chapter is a sympathetic but critical discussion of the social and economic interpretations of history given during the French Restoration by Guizot, Thierry and Mignet. In the third, the Utopian socialists are criticised for their " unscientific " assumption that their ideals would be realised as the result of " a happy accident ". In the fourth chapter, on Idealist German philosophy, there is an account of the dialectical element in dialectical materialism. The fifth and longest chapter is an elaborate vindication of the materialist conception of history as the basis of Scientific Socialism. Although there is a good deal of rather tedious criticism of non-Marxist Russian revolutionaries, the book is more pleasant reading than most Marxist writings, since Plekhanov was willing to indulge in a somewhat freer type of exegesis than is commonly met with in the successors of Marx and Engels. Nor have his serious political differences with Lenin led to his exclusion from the Marxist Canon. Indeed, it is hardly conceivable now that he ever could be relegated even to the Apocrypha, for, according to Mr. Rothstein in his informative introduction, Lenin wrote three years after Plekhanov's death that his philosophical works were " the best of all the international literature of Marxism ".

For all his freshness of style, however, Plekhanov does not diverge from Marx and Engels even on points that obviously require amendment. For example, he comes to the defence of Engel's notorious account of the negation of the negation in the *Anti-Dühring*. He is thus led to consider in all solemnity whether the grain of barley is negated by the whole plant that springs from it or by the stalk only as had been maintained by Mikhailovsky. " How " he writes (p. 113) " will Mr. Mikhailovsky have us deal with non-flowering plants ? Is he really going to leave them in the grip of the triad ? " Plekhanov also undertakes to defend Marxism from the charge that it immorally makes what will necessarily happen in the future the criterion of what we ought to do now. Like Hegel, however, he only succeeds in showing that some so-called idealists irresponsibly ignore relevant social facts. Indeed, he either confuses or deliberately assimilates scientific theory and moral aspiration when he writes : " From the point of view of Marx it turns out, consequently, that ideals are of all kinds : base and lofty, true and false. That ideal is true which corresponds to economic reality ". As the result of this identification of " true " and " lofty " he is able to conclude that Marxism is " the most idealistic theory which has ever existed in the history of human thought " since what will necessarily win the future is the loftiest thing of all, " the great cause of love " (p. 221). It is easy to see from this book that Plekhanov succeeded in expounding the Marxist philosophy

in fresh and vivid language, and that he had a lively appreciation of the objections that a Marxist philosopher ought to meet. To meet them, however, was quite beyond his power.

H. B. ACTON.

A Philosophy of Mathematics. By LOUIS O. KATTSOFF. The Iowa State College Press, 1948. Pp. 266.

THIS work would have been better named " A Source Book of the Philosophy of Mathematics ". After introductory chapters on suggested definitions of mathematics and various theories about the nature and objects of mathematics, Professor Kattsoff gives an account of the attempts of Frege, Russell, Pasch and others to define the natural numbers. He then explains the extensions of the number system, including the theory of transfinite numbers, and goes on to outline the views of Russell, Chwistek, Hilbert, Church, Brouwer, Gödel and Mannoury about the foundations of mathematics. There are some chapters at the end which profess to give the author's own views, but such merit as the work possesses is to be found in the summaries of books and articles which are not easily accessible. Unfortunately the author has not taken sufficient care in the writing of his manuscript and the correction of his proofs. On p. 77, for example, he ascribes to Tarski a book called *Einführung in die Mathematische Denken* (apparently a conflation of a book by Tarski and one by Waismann); and on p. 138 he writes: " The λ here, and as the D_x of calculus, are some of the abstractions A_m referred to by von Neumann". Irritating misprints such as " analagous " are frequent, and sometimes the formulae are affected, *e.g.*, on p. 243, where one of De Morgan's rules is stated incorrectly. Nor do Professor Kattsoff's comments inspire confidence. On p. 195, for example, he writes: " Since it is the contention of the author of this book that logic and mathematics do tell us something about reality, the Gödel theorem tells us that there can never be a complete and final theory of reality, *i.e.*, metaphysics. This does not lead to mysticism, but to an evolutionary scheme of reality. " It is a pity that he did not confine himself to the task of making accurate summaries with the minimum of comment required for showing connexions. A book which dealt in this fashion with the field which he tries to cover would undoubtedly be very useful.

WILLIAM KNEALE.

The Happy Issue. By WARNER ALLEN. Faber & Faber Ltd. 1948. Pp. 288. Price 12s. 6d.

AN unexpected mystical experience has driven the author firstly to reflect upon its nature and upon the question of what the world must be like to account for its occurrence, and secondly to delve into the writings of the classical mystics and compare their accounts with his own. The result is a book of varied contents. There is some genuine philosophising which commands respect; there are interesting and instructive accounts of Zoroastrianism, Manichaeism, Quietism (to cite a few examples)—and three chapters on Pascal deserve special mention; there are also rhetorical and highly figurative passages which I for one do not know quite what to make of: they have their own impressiveness, but they seem sometimes to obscure rather than clarify, and I occasionally wonder whether metaphors are being used as refuges rather than as ornaments.

Mr. Allen records four main convictions which his vision has brought: (*a*) Each of us has (or is) a permanent perceiving self or Ego which is the subject of all our experiences and which is sharply distinguished from the "objective", introspectible self which is composed of experience and which Mr. Allen calls an Emauton; the Ego's existence is explicitly defended against Hume and Bradley. (*b*) This Ego is eternal, *i.e.*, it has a kind of timelessness and changelessness which is nevertheless not static but dynamic. (*c*) The Ego is divine, is literally identical with God; hence no two individuals differ with respect to their Egos, but only in their Emautons. (*d*) God, however, is transcendent as well as immanent, for the evil in the Emauton is seen in stark contrast with the perfection of the Ego. The three main characteristics of Mr. Allen's interpretations of mysticism may be expressed as follows: (1) In ordinary experience, the Ego is aware of external objects and of the Emauton; in mystical experience, he holds, the Ego becomes conscious of *itself*. (2) The conceptions of God's immanence and transcendence must be held together, in contrast with doctrines which one-sidedly emphasise either to the exclusion of the other. (3) The empirical world is not either illusory or something to be shunned, but an essential part of the divine purpose, and the mystic's task is actively to better it so that the Emauton may ultimately become worthy of the divine Ego on which it depends. In this task and in the attitude towards it which mysticism supplies is to be found the realisation that the world "makes sense" and the understanding of the function of evil—in fact "the happy issue out of all our afflictions".

Mr. Allen rightly observes that one of the major stumbling-blocks in his view will be for many people the identity of the Ego with God. It is of course difficult for one who is not himself a mystic (and perhaps also for one who is) to distinguish between the content of a mystical experience itself, or what is immediately certified by it, and the interpretation subsequently placed upon it. The mystic would presumably dig in his heels on the former but admit the latter to be fallible. I, however, for reasons quite unconnected with mysticism, should want to dig in my heels on the following propositions: (*a*) that the "knowing self" and the "acting self" are one and the same; (*b*) that some of my actions are morally bad; and (*c*) that God never does any morally bad actions. From which it follows that my "knowing self" is not identical with God. And I find it very difficult to believe that if there is an enduring Ego it does not change for the better or the worse with the accumulation of experience and actions. I wonder, in fact, whether, even accepting mystical experience as of the highest possible evidential value, it warrants anything further than the belief that in some sense God is to be found within us (which is theologically orthodox enough); but there is a wide gulf between this and the assertion that our Ego literally *is* God.

The book is well indexed, but references for quotations are often inadequately given.

GEORGE E. HUGHES.

Essays in Philosophy and other Pieces. By A. D. RITCHIE. London: Longmans, Green & Co. Ltd., 1948. Pp. 208. 12s. 6d.

PROFESSOR Ritchie has assembled a number of essays on a variety of topics, logical, biological, theological, biographical, ethical and political. In the

logical group his main concern is in illustrating and developing Collingwood's "logic of question and answer", in criticising Russell's criticisms of Aristotle, and in exhibiting the failings of "logical positivists", their misapprehensions about scientific method (especially their Phenomenalism) and their assumption that ordinary language is a kind of calculus. Exact translation is possible only in mathematics. If emotional language is disreputable, no language (not even mathematicians') is reputable. Prof. Ritchie gives a 'general definition' of language as symbolising 'primarily an attitude or type of behaviour of the speaker', and secondarily the 'appearances which are themselves signs standing for the things or persons' which are being considered. (Prof. Ritchie does not discuss the three-fold significance of 'sign' which this definition implies.) 'When you make an assertion at least you desire (Emotion No. 1) your hearers to assent (Emotion No. 2).' This somewhat evangelistic tie-up of asserting with assenting reminds one of those who suppose that all ethical pronouncements are concerned with converting the fallen or discouraging the converted from backsliding. Very often people seem to 'say' things, at least, without caring very much whether their hearers agree or not, and contentious talkers are disappointed if their assertions bear no fruit of controversy. In a later discussion (in dialogue form) Socrates is represented as saying that, being ignorant himself, he disliked even the semblance of knowledge in others. Might one not 'assert' this—perhaps believe it even—without any very intense desire for the nodding of heads? Those, too, who prophesy death and destruction are often, but not invariably, vexed when controverted. "I would gladly be convinced of the error in my words" is not always a lie.

On probability, Ritchie commends Ayer's treatment of it in terms of confidence in a proposition and willingness to rely on it in practice, but argues that by so treating them Ayer has—unwittingly—put statements of probability into the same class as statements of what is good, ugly, etc. (He does not mention the 'behaviouristic' analysis of 'confidence' suggested by Ayer's treatment.) 'Any type of assertion about truth, falsity, or probability is an assertion of value and will be meaningless if assertions of value are meaningless.' Some may doubt the value of a refutation whose truth-value rests at all on the assumed univocality of 'value' in this context. Ritchie himself offers, as an explanation of the meaning of "it is true that . . .", the phrases "It is a fact that, . . ." "It happens that, . . ." or "I expect that, . . ." or "I am confident that, . . ." These are not, I think, meant as alternative ways of explaining different uses of "It is true that, . . .": the element of expectation, at least, is taken to be common to all uses. I am not sure, therefore, that Ritchie's explanation explains the case of the liar convinced of the falsity of his statements, which nevertheless happen to be true; the man who says truly (though not truthfully) that so-and-so is such while having complete confidence that so-and-so is anything but such.

Prof. Ritchie regards Collingwood's contribution to logic as important, though in need of restatement and amplification. He agrees that precise knowledge comes of asking precise questions and in no other way, but adds that questions, to be of service, must be such as to determine methods of examination and inquiry, and must be reasonably few in number, so that they can be answered one by one. He points out that the possibility of formulating precise questions depends on the previous answering of less precise questions, and that in the initial stages of an inquiry questions are necessarily both vague and numerous.

Absolute presuppositions are taken to be of the same type as the Kantian

categories, which Ritchie regards as determining (for those who accept them) their questionnaire-forms: he does not agree with Collingwood that such presuppositions 'cannot' be questioned or amended but can only be presupposed (or entered in the metaphysical archives as having been presupposed).

One last point. Ritchie does not tell us how to decide whether one question is more 'precise' than another: the matter seems obviously decidable only as between questions of the same type or sort. May not all this talk of precision blind men to the work of the 'spell-breakers', whose achievement it is, not to reformulate the old incantations more precisely, but to transform them out of all recognition?

R. J. SPILSBURY.

A Review of the Principal Questions in Morals. By RICHARD PRICE. Edited by D. DAICHES RAPHAEL. Geoffrey Cumberlege, at the Clarendon Press, Oxford. Pp. 301 + xlvii. Price 15s.

IT is pleasant to have Price's book once more available (there has been no edition since 1787) and in a format unusually readable, for these straitened days. Selby-Bigge's *British Moralists* naturally reflects the philosophical interests of his own time; now that attention is so much directed towards the logic of ethics, his extracts, mutilated at precisely the points which most concern the contemporary ethical theorist, are no longer a satisfactory substitute for the original texts.

The text is that of the third edition, with footnote quotations from the first and second editions whenever the variations are significant. It includes the *Additional Notes*, the *Dissertation on the Being and Attributes of the Deity* and the *Preface* to the first edition. So far as one can judge without detailed collation, the text has been carefully prepared and printed.

Professor Raphael's introduction is primarily a critical summary. So far as it is a summary, one scarcely finds it necessary, with the text in front of us; a more detailed table of contents would have been of greater usefulness. The criticism (as its brevity makes almost inevitable) is of a somewhat arbitrary kind. "The reply to scepticism is good" (p. xviii); "I think that Price's brief discussion of freedom is good" (p. xl)—remarks of this sort are uncomfortably reminiscent of a schoolmaster distributing marks.

In his anxiety to emphasise Price's modernity, Professor Raphael somewhat minimises his indebtedness to his predecessors; or, at least, does very little to help us understand how intimately Price's work relates to the philosophical controversies of his own time. To take one point, it should surely have somewhere been indicated that the main argument on pp. 28-9, pp. 50-1, pp. 19-20 is paraphrased (with occasional direct quotation) from Cudworth's *Eternal and Immutable Morality*. Admittedly, Price several times makes his own acknowledgment to Cudworth but not usually in such a way as to show precisely in what his indebtedness consists. For example, Price vaguely refers to a distinction which "some" have made between "material" and "formal" goodness (p. 177); in his preface, Professor Raphael wonders who this predecessor might be; the distinction (in this language) is, of course, central in Cudworth's book. And it is not enough to say that Cudworth "caused Price to see that ethical theory ought to be grounded on epistemology" (p. xiv). Substantially, Cudworth *supplied* Price with an epistemology.

Sometimes, indeed, Price was sufficiently intimidated by the empiricism of his own time not to commit himself quite fully to Cudworth's epistemology. If Professor Raphael had realised that Price was not restrained by disbelief but by a species of prudence, he might have been less puzzled (p. xv) to understand why Price refers his readers to theology for a description of the status of universals; the reference is certainly to Cudworth's view that God is the permanent sustainer of universals, which we know only in so far as we participate in the activities of the Divine mind. And Cudworth is only one, although perhaps the most influential, of Price's predecessors; Price's exact relation to Clarke and to Shaftesbury would have repaid investigation, not merely as a piece of historical reconstruction, but as a means of illuminating the structure of Price's argument. Perhaps what would finally emerge is not so much the modernity of Price as the antiquity (in essentials) of contemporary deontology.

Of course, an edition is not a commentary, and space is limited. The question is how it ought to be filled. On that tastes differ; if we want a repository of historical knowledge bearing on the text, this edition will not satisfy us (it has not even a bibliography). But, in any case, we remain grateful for the re-appearance of the text, and for Professor Raphael's patient attention to it.

J. A. PASSMORE.

An American Utilitarian: Richard Hildreth as a philosopher. By MARTHA M. PINGEL. New York: Columbia University Press (London: Geoffrey Cumberlege), 1948. Pp. xi + 214. Price $4.50, 16s.

MISS Pingel has collected together two short unpublished treatises and half a dozen journalistic polemics by Richard Hildreth and published them with a short account of his life and of his published *Theory of Morals*. Hildreth, who was born in Massachusetts in 1807, was by turns, lawyer, journalist, historian, philosopher and anti-slavery propagandist. To this last activity he owed his appointment by Lincoln in 1861 to a minor diplomatic post in Italy, in which country he died three years later.

Hildreth emerges from Miss Pingel's narrative as a typical Utilitarian, intellectually honest, not overendowed with originality or depth, confident in the possibility of applying a rather naively conceived scientific method to moral and social problems and actuated by a deep love of mankind and an equally sincere hatred of the theological and patriotic cant that passed in his day for respectability. His own writings confirm the verdict. Both the *Theory of Wealth* and the *Theory of Taste* are insipid and flat, while the polemics in which he defends his published *Theory of Morals* show a certain pungency and cogency of argument. He is, however, as far below Swift, Voltaire or Paine in the polemical style as he is below his master, Bentham, in sustained and original thought.

To judge from Miss Pingel's quotations, the *Theory of Morals*, while setting out to remove contradictions from traditional Utilitarianism, has only the opposite effect. We are told that "Happiness, in Hildreth's system, is a positive state, consisting of many ingredients of which pleasure may be one. Pleasure, on the other hand, is a negative state; simply the cessation of pain". To insist on using two words for two things is laudable economy of language; but the benefits are appreciably lessened when we hear on the very next page that "happiness, in any sense in which it is practically an object of pursuit, consists merely in the avoidance of or escape from present pains". Like Bentham, Hildreth laid more

emphasis on reward than on punishment and saw that it is futile to try "to inspire with sentiments of virtue and benevolence great masses of men who are at the same time in a state of starvation".

The *Theory of Wealth* is remarkable for containing an attack on the generally accepted labour theory of value, and Hildreth seems to have been one of the first to point out that improved agricultural techniques might knock the bottom out of Malthus' theory of the pressure of population on the soil. The *Theory of Wealth* is followed by forty-eight pages on aesthetics, entitled *Theory of Taste*. In the six reprinted polemics Hildreth's main objects were to defend the autonomy of morals against the prevailing view of the New England divines that actions are right because God commands them, to show that the appeal to the Bible as a source of moral guidance must, in the end, be an appeal by each interpreter of the Bible to the infallibility of his own interpretation and to defend himself against the charges of atheism and impiety that were brought against him for having dared to point out that men who were neither Jews nor Christians have had moral standards and that men's moral standards have been widely divergent. What Hildreth says here must have been well worth saying in Boston in the eighteen forties, but his polemics have little of the quality that makes such writing enthralling after a hundred years.

Hildreth's main claim to fame, apart from his contribution to the anti-slavery cause, was to have translated Dumont's abstract on Bentham's *Theory of Legislation* and thus to have popularised Utilitarianism in America. For this he deserves a mention in any history of American philosophy. That his own philosophical work deserves to be rescued from oblivion is much more doubtful.

P. H. NOWELL-SMITH.

Essays in Political Theory presented to George H. Sabine. Edited by MILTON R. KONVITZ and ARTHUR E. MURPHY. Cornell University Press (Geoffrey Cumberlege; Oxford University Press), 1948. Pp. viii + 333.

THIS book contains a collection of seventeen essays by colleagues and former students of Prof. Sabine presented to him on his retirement, together with a bibliography of Sabine's writings.

About half the essays are concerned with matters of historical fact.

J. A. O. Larsen examines the earliest known uses of the word "demokratia", and gives reasons for supposing that it came into use in Athens in the fifth century as a name for the constitution of Cleisthenes.

Max H. Fisch summarises some opinions of Vico on the interpretation and origins of Roman Law.

R. R. Palmer gives a useful survey of various attempts made by the French Revolutionaries to put into effect the doctrine of individualism, defined as "a philosophy which seeks to emancipate the individual from all involuntary dependency on collective groups".

Charles M. Wiltse writes a short history of the doctrine of "states' rights" in American political controversy.

Carl Wittke gives a biographical sketch of the nineteenth-century Socialist leader Wilhelm Weitling, with a very brief account of his opinions.

Milton R. Konvitz contributes an appreciative summary of John Morley's " Compromise ".

Albert R. Chandler reviews " the political typology of Moeller van den Bruck ".

Robert E. Cushman examines the use made in recent decisions of the U.S. Supreme Court of the principle, first laid down by Mr. Justice Holmes, that the right of freedom of speech may be restricted by the government only when the exercise of this right involves a " clear and present danger ".

Four of the essays discuss the opinions of celebrated philosophers.

Glenn R. Morrow gives a careful review of the use of the words " nomos " and " physis " in fifth-century Greek philosophy, and in Plato. He argues that before Plato these were opposed terms ; Plato's conception of Nature as constituted by the Forms enabled him to remove this opposition, and brought him in the " Laws " to the notion of Natural Justice, and nearly to the notion of Natural Law, by which Natural Justice is expressed. This line of thought, says the writer, must have had an important influence on the later Stoic doctrine of Natural Law.

Francis D. Wormuth sets out to combat the idea that Aristotle was an enthusiast for the " Rule of Law " in any of its usual senses. A ramble through a number of relevant passages shows that Aristotle did not consistently maintain, either that statute law is always to be preferred to the judgment of individual judges, or that there is a " universal law " superior to all statute law.

Arthur E. Murphy examines the account of Locke's political philosophy given in F. S. C. Northrop's *Meeting of East and West*. Northrop maintains that Locke based his theory on the notion of atomic, independent mental substances, derived from Newton's physics ; that he " defined the good for the individual in terms of nothing but freedom of the mental substances " ; and that in consequence he over-valued thinking and under-valued feeling, and failed to take adequate account of the social bonds between men. Murphy, in a lucid and forcible essay, shows that this is a distortion of Locke's doctrine, which was based, not on the idea of a mental substance, but on that of a rational moral person, standing to his fellows in a moral relation between free men who have a common understanding of law and right. The writer fears that Northrop's misinterpretation may weaken our respect for the liberal tradition derived from Locke, by obscuring the notion of responsible moral freedom on which it is based.

Katharine Gilbert is concerned with a criticism of the theories of Croce made by Ruggiero, *viz.*, that " Croce's historicism leaves no room for the justification of a genuine moral act ", and that " the lack of transcendent values means the absence of the vital disequilibrium between experience lived through and the will to good ". The writer gives a careful and fair review of many of Croce's pronouncements on this matter. She finds that there is much in his account of moral decision which needs to be clarified, but it appears that after all he does recognise a transcendent value of a kind, *viz.*, the ideal of " liberty " ; this ideal is, however, vaguely and elusively described. Croce does admit a distinctively moral plane of action, and exhibits it in concrete historical examples, but he does not maintain a clear and consistent distinction between " moral " and " economic " choices.

Of the five papers which discuss philosophical problems, three deal with the same topic, the " objectivity " of moral and political judgments.

The most interesting is that of Julius R. Weinberg, who argues on

these lines: Hume was right in saying that no moral proposition is implied by any set of factual propositions. But it does not follow that moral judgments "rest on sentiment alone". It remains possible that moral properties can be discerned in actions or states of affairs which we observe or imagine. He goes on to make what seems to me a valid and important point: we are able to criticise moral judgments, and by applying certain tests to distinguish pseudo-moral attitudes from genuinely moral attitudes; but on a theory such as Hume's there is no room for this distinction. Weinberg suggests that moral judgments are not based either on inferences or on intuitions of any simple property or strictly definable complex of properties. They are analogous to judgments about material objects, in that, without being able to give an explicit definition of goodness, we can specify the kind of evidence which would be favourably or unfavourably relevant to the proposition "X is good".

Frederick L. Will, on the same topic, admits that value-judgments "are not certifiable by the techniques of demonstration and verification cultivated in the mathematical and empirical sciences respectively". Yet, he says, they are unlike mere tastes (in drinks, for instance), because "some things have demonstrated well their general capacity to bring satisfaction or happiness into human life in such a way that we know that they are a general and enduring human good"; and thus, even if a man has no appetite or appreciation for them now, we can recommend them to him as "good" in the confidence that, if given their chance, they will "prove themselves in his own experience". The objectivity of value-judgments then depends on the fact that there are some things which all or most men are capable of appreciating in much the same degree. On this ground we can draw up, not certain, but probable, "objective" rules for the guidance of aesthetic taste, and, similarly, can recommend particular social institutions, such as democracy, as ultimately more satisfying than the alternatives to them, even though all men do not actually desire them at present. The writer does not indicate how he would show that the required uniformities of moral appreciation exist; this seems a very difficult enterprise, when one remembers that serious moral and political problems are almost always problems of *relative* valuation.

G. Watts Cunningham's paper is so vaguely written (*e.g.*, "The moral agent is the basic moral fact") that I have had much difficulty in making out his argument. He seems to be saying that, because we must use reason to determine the facts of a situation in which we have to act, because we do not act in a vacuum, but every action involves the selection of appropriate means and commits us to foreseeable consequences, therefore "reasoning determines moral good and evil". If this is his argument, I do not think any ethical subjectivist need be disturbed by it. His paper also includes a discussion of democracy, which starts from the proposition that "what is meant by the word democracy as used by the professed democrat may be shortly described as the way of life within which the 'welfare of the people' is the objective of both individual and group behaviour". This proposition seems to me blatantly false.

Bertram Morris's essay on *The Substance of the Social Contract* tells us that "Liberalism in the modern world comes to signify liberty, equality and fraternity", and "The contract is achieved in those institutional forms which further the life of a culture", and more in the same vein.

Henry Alonzo Myers' contribution is a sort of sermon, in which he exhorts us to remember that men cannot live by compromise alone, and

to give "heroism" its proper value. In "heroism" he seems to include a willingness to take risks, an unwillingness to compromise, and a preference for high intensity of experience over long duration. I am not convinced that "heroism" and "the way of compromise" are properly antithetic; Count Bernadotte took plenty of risks. And I certainly do not believe, as Myers does, in a general law by which "sorrow is the price of joy", and "each pays for what he gets".

The collection as a whole is of very variable quality. It will probably be of most value to the historian of political thought.

C. H. WHITELEY.

La Découverte de Soi. By GEORGES GUSDORF. Pp. 510. Presses Universitaires de France.

THIS book is representative of a type of philosophical inquiry which is now common in France but is very uncommon in this country; the prevailing philisophical tradition and method in the two countries diverge again to-day as they have so often in the past. To the contemporary English philosopher the problem of the self generally presents itself primarily as a problem of logical analysis; M. Gusdorf treats it as primarily a moral problem and secondarily as a problem of moral philosophy. Whenever there is speculation about the human condition something is said about the nature of the individual personality and the means of self-knowledge; and it is these speculations which M. Gusdorf collects and arranges. The wide range of sources from which the material is drawn makes the book illuminating; it offers an escape from the narrow circle of ideas within which academic epistemology has confined the problem of self-knowledge by drawing on the speculations of scientists, great writers and whoever else has thought seriously of self-knowledge as constituting a problem. Not the least valuable part of the book are the quotations from writers such as Nietzsche, Valéry and Gide which represent protests against the conventions of ordinary speech; one is made to realise how transient and alterable these conventions are and how frail are the analogies on which they rest.

The book begins with Socrates' call for self-knowledge and examines the common underlying assumptions of such professional self-knowers as St. Augustine, Montaigne and Rousseau; it illustrates the growth of the ideal of sincerity as the discovery of self and, arising out of this ideal, the development of the *Journal Intime* in the nineteenth century, and the later collapse of this ideal of 'conscience miroir' with the advent of Freud and others, and the tendency to say that we only come to know—or, better, know about—ourselves indirectly as a by-product of choosing and acting. M. Gusdorf quotes Kierkegaard frequently and is clearly somewhere on the existentialist stream; his writing is sometimes repetitive and obscure in this manner—a manner which is so remote from the new English clarities and simplicities. The book is too long and too dense to be adequately summarised, and the argument is discontinuous; although difficult to read at length, it contains many paragraphs which are intrinsically stimulating and will certainly interest all those who are curious about the new idiom of French philosophy.

S. HAMPSHIRE.

The Renaissance of the Individual. By KURT LACHMANN. Pp. xvi + 143. London : Charles Skilton Ltd., 1947. 7s. 6d.

DR. LACHMANN's thesis is that law and not the State should be sovereign. The laws of particular states protect individuals from each other within their boundaries. They should be supplemented by an inter-State or universal law to protect individuals from their governments. At present most persecuted individuals can appeal for redress nowhere outside the legal boundaries of a national State. Their plight may rouse 'the conscience of the civilised world' but this, at present, has no organ of legal action. Its protests tend to be ineffectual and even avoided for fear of political broils. Charitable relief may be given by conscience-stricken states to refugees, but the status of a refugee alien is ambiguous and unenviable. Moreover, the persecuted do not want charity, but justice.

The remedy for this situation, Dr. Lachmann thinks, is the creation of a Supreme Court of International Justice, manned by 'State-independent' judges, to administer a universal law based on certain fundamental principles of justice 'valid for all individuals' (p. 3, and ch. 4). This would supplement, not supplant, present international law which regulates the relations between sovereign states but has no power to interfere in their 'internal affairs', *i.e.*, to criticise, much less adjudicate upon their treatment of their own members. Dr. Lachmann's legal models are, rather the Supreme Court of the U.S.A. and the Judicial Committee of the British Privy Council. The international court must, however, be separated from national legislatures. It is not enough that its principles should be written into national constitutions. For this subordinates them to national sovereignty and does not prevent their repudiation by later governments.

Dr. Lachmann assumes (perhaps rightly) that no civilised adult needs to be *taught* the principles of universal justice. These may be explicitly denied (though, generally, the most despotic government pays, as we say, lip-service to them in some guise) and flouted in practice, but no one can plead ignorance of them. It is a pity, though, that Dr. Lachmann expresses this fact in the mysterious language of a 'natural law' written in the hearts of men. A 'natural law' which so frequently fails to work is not very sensibly called a law at all. But there is no doubt of the moral situation to which Dr. Lachmann invites our attention. No doubt genuine disputes could occur about whether or not an individual were being unjustly treated by a government. It should not be *assumed* that governments are always wrong. What is important is to establish a Forum on which such disputes can be debated by experts empowered to make a binding decision. Not even this, of course, could guarantee perfect justice always. That it could is, perhaps, a natural delusion of internationally-minded lawyers. Even 'State-independent' judges may be fallible. But since they would undoubtedly add another layer of protection to the individual, I should rejoice, with Dr. Lachmann, to see them appointed. Unfortunately, the proceedings at Lake Success and elsewhere do not give much prospect of early rejoicing.

Dr. Lachmann goes on to discuss federal union as the political basis of world organisation and the moral aspects of economic justice. His discussions are fresh and interesting and characterised by great moral earnestness. It is obvious that these problems are, for him, of more than theoretical interest. Having chosen exile rather than subjection to a despotic government, Dr. Lachmann, rightly, looks forward to a world in which such a choice will not be necessary. For a citizen of the world may change

his residence but will never be an exile. He must live, however, which is why legal, political and economic problems cannot be separated. Universal law implies global economy, with reasonable freedom of movement across frontiers. But whatever solutions of our problems are sought and found, Dr. Lachmann has eloquently reminded us that the material of political experiments are human lives and, consequently, governed by considerations which do not apply to guinea pigs.

MARGARET MACDONALD.

Physical Science and Human Values. A symposium with a foreword by E. P. WIGNER. Princeton University Press, 1947. $3.00.

THIS book is a record of a conference held at Princeton University in 1946. A group of physical scientists step self-consciously from their Ivory Towers into the arena of social responsibility. Their problems are varied. Some of them are problems of "social engineering"—*e.g.*, how teaching methods and equipment in American High Schools must be modified in order to cater for "scholars" and "specialists" as well as for the non-academic, how large technological laboratories can be adapted to problems of basic research under peace-time conditions. Then there are the theoretical questions which we would expect in such a gathering concerned with the relation between science and ethics, and the problems of policy occasioned by the peculiar position of nuclear physicists in modern communities.

The chapters take the form of a paper followed by contributions to discussion. This means that many points are raised but few developed in any detail. Of particular interest to philosophers is Professor Northrop's paper on "The Physical Sciences, Philosophy and Human Values" and Professor Bridgman's paper on "New Vistas for Intelligence". Northrop maintains that ideological differences, such as those between Russia and America, derive from different "philosophies of nature" which in their turn are the reflection of current views of scientific method. For example, the American Declaration of Independence enshrines an ideology derivative from the Lockean theory of the person who has no "intrinsic social relations" with other persons. This theory in its turn is the outcome of Locke's philosophy of nature which was necessitated by the Galilean-Newtonian conception of scientific method. Hooker's theological polity and Marxist ideology have similar links with different philosophies of nature and different conceptions of scientific method. He then asks whether differences in ideology can be reconciled by rational methods. We are astounded to hear that Plato and Socrates in the *Republic* have given the answer. "Only those who have passed through the hypotheses of the mathematical and physical sciences can arrive at the idea of the good which one has a right to say holds for everybody." How reassuring for the assembled physicists!

Bridgman advocates the attitude of "no holds barred" and "the method of intelligence" in dealing with problems of human relationship in order to arrest the "chain-reactions" let loose by the breakdown of belief in absolutes. By this he seems to mean that investigators in this field must be prepared to examine and question their own presuppositions; they must define social concepts operationally and examine how "absolutes" function in social situations. He suggests several interesting

lines of enquiry—*e.g.*, " How far the common assumption is justified that men are fundamentally alike intellectually and can come to agreement ", " what are the minimum codes of conduct, which, if universally accepted, would lead to a stable society in which conditions of life would be sufficiently tolerable ". The main problem in these types of investigation is how to set about them. When Bridgman suggests as a start in his campaign for " mental rehabilitation " that groups of five people, with different intellectual interests, should be shut up and told to hammer out what they could agree upon " in matters of ordinary social concern " and to find the focus of their disagreement, when they cannot agree, we feel rather uneasy. We are reminded of Leibnitz's dream of mathematicians settling moral disputes once they have agreed on their universal language.

However, Bridgman's paper and the ensuing discussion suggest lines of enquiry for philosophers who are tired of " common-room ethics " and equally unimpressed by persuasive attempts to show that ethics is not a subject worthy of the philosopher's attention. But this type of approach to ethics requires a thorough grounding in the social sciences and in psychology, to which most of us seem to have some kind of a " resistance ".

RICHARD PETERS.

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VIII.—NOTE

JOINT SESSION OF THE ARISTOTELIAN SOCIETY AND THE MIND ASSOCIATION AT BANGOR 8TH TO 11TH JULY, 1949.

Accommodation will be provided at Neuadd Reichel (including Annexe), Ffriddoedd Road, and Bala-Bangor Theological College, Bangor, and the meetings will be held in the Junior Common Room at Neuadd Reichel.

The inclusive charge for board and lodging from Friday dinner to Monday breakfast will be £2.

There will be a charge of 15s. as a Registration Fee for membership of the Joint Session, with the Volume of Proceedings, and of 5s. for membership without the volume.

Payment of the Registration Fee and of the charge for accommodation should accompany applications, which should be made to

Mr. D. A. Rees,
Department of Philosophy,
University College of North Wales,
Bangor, Caernarvon.

FRIDAY, 8th July.

at 5.30 *p.m.*
Annual General Meeting of the Mind Association.

at 8 *p.m.*
Address by Prof. H. D. Lewis, "Revelation and Art."

SATURDAY, 9th July.

at 10 *a.m.*
Symposium "Causal Laws in Psychology."
Mr. B. A. Farrell. Mrs. Margaret Braithwaite. Prof C. A. Mace.

at 8 *p.m.*
Symposium "Is there Knowledge by Acquaintance?"
Mr. H. L. A. Hart. Mr. G. E. Hughes. Prof. J. N. Findlay.

SUNDAY, 10th July,

at 10 *a.m.*
Symposium "Science and Politics."
Mr. Rush Rhees. Mr. T. D. Weldon. Mr. P. Nowell-Smith.

at 8 *p.m.*
Symposium "What are the Distinctive Features of Arguments used in Art Criticism?"
Mr. A. H. Hannay. Mr. John Holloway. Miss M. Macdonald.